The Econometrics of Cartel Overcharges

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March 21, 2011
REVISED August 10, 2011

Cahier n° 2011-18

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Résumé : Connor et Lande (2006) survolent la littérature sur les majorations de prix imposées par les cartels et concluent à une augmentation moyenne variant entre 31% et 49%. Considérant un échantillon plus grand, Connor (2010b) trouve une médiane de 23,3% pour tous les types de cartel et une moyenne de 50,4% pour les cartels dont les majorations de prix estimées sont positives. Cependant, les échantillons utilisés dans ces études sont constitués d'estimations et non pas d'observations directes. De ce fait, ces échantillons héritent possiblement d'erreurs de modélisation et d'estimation, ainsi que d'un biais de publication. Une analyse sommaire des majorations dans l'échantillon de Connor révèle une distribution asymétrique, de l'hétérogénéité et la présence d'observations aberrantes. Ainsi, au-delà du fait que les estimations d'augmentation de prix par les cartels sont potentiellement biaisées, l'estimation d'un modèle par MCO avec de telles données sans un traitement adéquat de l'asymétrie, de l'hétérogénéité et des données aberrantes produirait des résultats déformés. Nous réalisons une nouvelle méta- analyse dans le même esprit que celui de Connor and Bolotova (2006), mais en proposant une prise en compte adéquate des problèmes mentionnés ci-dessus. Après correction du biais d'estimation, nos résultats suggèrent que la moyenne des majorations de prix estimées est de l'ordre de 13,6% avec une médiane de 13.6% pour les cartels dont les estimations de majoration de prix se situaient initialement entre 0% et 50% et de l'ordre de 17,5% avec une médiane de 14.1% pour tous les types de cartels.

Abstract: Connor and Lande (2006) conducted a survey of cartel overcharge estimates and found an average in the range of 31% to 49%. By examining more sources, Connor (2010b) finds a median of 23.3% for all type of cartels and a mean of 50.4% for successful cartels. However, the data used in these studies are estimates rather than true observations, since the true illegal profits of cartels are rarely observable. Therefore, these data are subject to model error, estimation error and publication bias. A quick glance at the Connor database reveals that the universe of overcharge estimates is asymmetric, heterogenous and contains a number of influential observations. Beside the fact that overcharge estimates are potentially biased, fitting a linear OLS model to the data without providing a careful treatment of the problems raised by the publication bias, outliers, asymmetry, and heterogeneity will necessarily produce distorted results. We conduct a meta-analysis of cartel overcharge estimates in the spirit of Connor and Bolotova (2006), but providing a sound treatment of the matters raised above. We find for cartels with initial overcharge estimates lying between 0% and 50%.a bias-corrected mean overcharge estimate of 13.6% with a median of 13.6% and for all cartels of all types a bias-corrected mean of 17.5% with a median of 14.1%.

Mots clés : Amendes optimales ; cartels

Keywords : Optimal fines ; Cartels

Classification JEL : L41, K42

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[1] We thank Jimmy Royer and René Garcia for their comments on a previous version of this paper, but we remain solely responsible for its content.
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1. Introduction: the representative mean cartel overcharge

A cartel is a group of independent firms which collectively agree to coordinate their supply, pricing or other marketing policies in order to make larger profits than they would when "natural competition" prevails. Depending on the market of interest, the natural competition can be pure and perfect, oligopolistic or monopolistic, with firm strategies either centered on (static) short term profit maximization or on (dynamic) long term value maximization, the latter giving rise to non-cooperative "natural competition" industry equilibria that may in different aspects resemble collusive equilibria.[1] The price that would prevail absent the explicit cartel conspiracy is called the "but-for price". When a cartel succeeds at raising its price, the amount it charges in excess of the but-for price is called the "cartel overcharge". Firms may collude if the incremental payoff generated by the overcharge is more than sufficient to cover the cartel costs. This incremental payoff comes from the overcharge paid by those consumers who are able and willing to pay the higher price set by the cartel, from which the profit loss firms incur on the reduced sales must be deducted. The surplus that accrue to consumers who do not buy at higher prices than the natural competition level is a deadweight loss for society. As the social welfare is generally increased when more competition prevails, one must acknowledge that cartels in pursuing their goals are harmful for society.

Most advanced economies consider cartels as illegal and are endowed with antitrust policies, i.e. legislations aimed at deterring the formation of cartels. For example, the United States Sentencing Guidelines (USSG) recommends a base fine of 10% of the affected volume of commerce to a firm convicted of cartel collusion. To this base fine, another 10% is added for the harms "*inflicted upon consumers who are unable or for other reasons do not buy the product at the higher price*". This yields a fine of 20% that may also undergo some adjustments for aggravating and mitigating factors. The total financial fine ranges from 15% to 80% of affected sales. In addition, there is a possibility of incarceration for the individuals involved in the collusion. In the European Union (EU), the antitrust policy is implemented by the European Competition Commission. The amount of the fine takes into account the severity of the damages inflicted upon consumers as well as some aggravating and mitigating factors, but the total fine must not exceed 10% of the overall turnover or global sales of the firm.

Cohen and Scheffman (1989) argued that an increase of 1% of a price above its natural competition level usually results in a reduction of sales of more than 1%. Based on this, they concluded that *"at least in price-fixing cases involving a large volume of commerce, ten percent is*

[1] Hence some of those dynamic long term value maximizing non-cooperative "natural competition" stategic industry equilibria namely those that resemble collusive equilibria are at times refered to, somewhat improperly, as "tacit collusion" equilibria.

almost certainly too high". They claimed that *"The Justice Department's assertion that price-fixing conspiracies would typically result in a mark-up over competitive level of ten percent, or that the probability of detection of price-fixing or bid-rigging would be as small as ten percent is not supported by the available evidence. Both of these Justice Department assertions are especially untenable for conspiracies involving private for-profit purchasers. This conclusion has important implications because of the potential inefficiencies that may arise from overdeterrence."*

On the other hand, Connor and Lande (2006) conducted a survey of cartel overcharge estimates by examining more than 500 refereed journal articles, working papers, monographs, and books. They found an average cartel overcharge in the range of 31% to 49% and a median overcharge in the range of 22% to 25% of affected commerce. Based on this, they concluded that *"the current Sentencing Commission presumption that cartels overcharge on average by 10% is much too low, and the current levels of cartel penalties should be increased significantly"*. After collecting and examining a larger data set, Connor (2010b) concludes that *"...penalty guidelines aimed at optimally deterring cartels ought to be increased"*. Combe and Monnier (2009) performed an analysis of 64 cartels prosecuted by the European Commission and arrived at the conclusion that *"fines imposed against cartels by the European Commission are overall sub optimal"*.

Hence there are disagreements among economists, not only about how to optimally set the fine in cartel cases, but also about the magnitude of the overcharge. The first matter is primarily of theoretical nature. A well-known fining rule is given by the so-called Becker-Landes formula, which stipulates that the optimal fine is equal to the harm caused to society by cartels divided by the probability of detection (See Becker 1968 and Landes 1983). The proper definition of the probability of detection and conviction is itself subject to different interpretation. In turn, the harm caused to society is equal to the cartel overcharge plus additional social costs caused by the presence of cartels, namely the deadweight losses caused by cartels and the resources devoted to antitrust authorities for fighting cartels.[2] The current paper deals with cartel overcharges and provides an order of magnitude of the overcharge of the representative cartel.

The sample used for our study is an extended but qualitatively similar version of the one used in Connor (2010b).[3] The raw database consists of 1178 cartels, from which we exclude 58 cartels with missing information. This leaves us with a sample of 1120 cartels with overcharge estimates ranging from 0% to 1800%. We will refer to it as the Connor database or Connor sample. The mean overcharge estimate is 45.5%, but 49% for strictly positive estimates. This average is 20.6% for the cartels with overcharge estimates lying strictly between 0% and 50%, representing 70% of the sample. Estimates that are larger than or equal to 50% represent 22.6% of the sample,

[2] One may add other effects of cartels such as their impact on investment and employment, on entry and exit dynamics, on innovation and learning curve, etc.

[3] We sincerely thank Professor John Connor for generously making his database available to us.

and the average overcharge estimate for this subsample is 137.3%. A close look at the data show that the 49% mean overcharge is actually due to the presence of a small number of influential observations. For example, when the 5% largest observations are left out of the Connor sample, the average overcharge estimate drops from 49% to 32%. But the argumentation of Cohen and Scheffman (1989) based on the theory of mark-ups suggests that an overcharge of more than 10% is "beyond belief." This leads to question whether the majority of estimates available in the literature, and in particular those used in Connor (2010b), overstate the actual overcharges. Often, the decision to estimate the overcharge is made upon presumption of collusion. Therefore, the extent to which this presumption affects overcharge estimates is an important question to investigate.

To sharpen our intuitions, we re-examined the methodology that consists of converting a Lerner index into an overcharge. In many instances, Connor (2010b) used this methodology to obtain overcharge estimates by assuming that the situation that would prevail absent the cartel is perfect competition. However, product differentiation and other industy-specific factors may generate market power that allows firms to apply significant mark-ups over marginal cost. We show that not accounting for the presence of such mark-ups causes overcharge estimates obtained by conversion of Lerner indices to overstate the overchage by potentially large margins. Based on this result, one can reasonably suspect that any other estimation method is susceptible of introducing a particular type of bias. Indeed, the magnitude of some overcharge estimates in the Connor database and the simplicity of the estimation methods used in many papers make the presence of substantial estimation bias plausible.

One way to verify if overcharge estimates are biased is to perform a meta-analysis, as in the seminal paper of Connor and Bolotova (2006). In the meta-analysis, overcharge estimates are regressed on two groups of variables. The first group is comprised of variables that in theory could explain or influence the size of the true overcharge (e.g. the characteristics of the cartel), while the second group gathers variables that in theory should not, and therefore are susceptible of capturing an estimation bias (e.g. the computation method and the publication source). The study of Connor and Bolotova (2006) provides evidence that part of the variability of overcharge estimates is actually due to the computation method and publication source. Another problem in the data is, as mentioned above, the presence of influential observations or ouliers. Such influential observations must be excluded from the meta-analysis if one wishes to avoid distorting the estimated coefficients, while wishing to draw conclusions that apply for the vast majority of cartels.

We conduct a new meta-analysis by introducing three refinements with respect to Connor and Bolotova (2006). In a first step, we remove the cartels with alleged overcharge estimates that are larger than or equal to 50% as well as zero overcharge estimates representing 7.2% of the sample because the proportion of such estimates is highly susceptible of being affected by the publication bias. Second, we use a K-means analysis to separate the sample of overcharges into four "homogenous" groups. And third, we model the logarithm of overcharges as a linear function of the explanatory variables mentioned above, while assuming that the coefficients of variables that capture the bias vary across the clusters identified in the K-means analysis. This log-linear specification appears to be less subject to distortions caused by influential observations, and explains the variance of the overcharge estimates to a greater extent than the standard linear specification. Since the zero overcharges have been excluded, no log-of-zero problem arises. In the regressions, a Heckman type correction is used to eliminate the sample selection bias due to the exclusion of zeros and outliers.

It should be emphasized that the exclusion of zeros and outliers in the first step is motivated by two distinct and equally important reasons. First, such observations are more susceptible than the other observations of being affected by estimation and publication biases. Second, even if these observations were measured without bias, their inclusion in OLS regressions would distort the estimation results. Note that in the log-linear regression, the log of zero is minus infinity and thus, is the largest possible outlier. However, by removing the problematic observations, we create a sample selection problem. This well known and documented problem can be controlled by a Heckman-type regression.

Our results show that the bias captured by the estimation method and the publication source is substantial and economically significant. The bias-corrected estimates obtained from the meta-analysis by adequately neutralizing the effects of those variables suggest that the representative average overcharge estimate for cartels with initial overcharge estimates above 0% and below 50% (the bulk of cartel cases) is approximately 13.62% (with a median of 13.63%), while the average for all types of cartels is approximately 17.52% (with a median of 14.05%).

The rest of the paper is organized as follows. In Section 2, we review the existing overcharge estimation methods and briefly discuss their respective drawbacks. In Section 3, we review the overcharge estimates presented in Connor (2010b) and show why one can reasonably express some doubts about the quality of some of these estimates. We proceed by providing concrete examples where Connor presents as cartel overcharge estimates numbers described as competitive mark-ups in the original publications. We also illustrate the danger of converting Lerner indices into overcharge estimates without caution. In Section 4, we discuss the philosophy underlying our meta-analysis and the econometric models used. In Section 5, we present in details the preprocessing that the raw data in the Connor sample must undergo in order to avoid drawing misleading conclusions from the empirical results.

Section 6 presents the determination of bias-corrected overcharges and the main empirical results. Section 7 concludes, and an appendix presents the description of the data.

2. The Overcharge Estimation Methods

Let $\tilde{p}$ be the price imposed by the cartel and p be the but-for price, i.e. the price that would prevail absent the cartel. Then the cartel overcharge is given by:

$$\Delta = \tilde{p} - p \quad (1)$$

The overcharge expressed as a percentage of the but-for price becomes:

$$\delta = \frac{\tilde{p} - p}{p} \quad (2)$$

While the cartel price $\tilde{p}$ is observed, the but-for price p needs to be estimated.

Many authors acknowledge that overcharges represent the bulk of the damages caused by cartels. This explains why the largest body of the economic literature on cartels has been devoted to their price effects. The methods often used by academic researchers and forensic economists to estimate the cartel overcharge can be summarized into 5 groups: price before/after the conspiracy, price during a price war, yardstick method, cost-based method, and econometric method. Each of these approaches is briefly explained below.

2.1 The " before-and-after" Methods

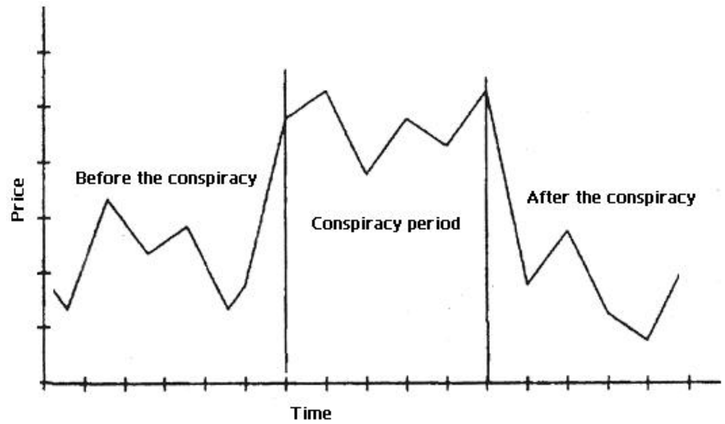
This method is based on the comparison of the price during the alleged cartel period with the price before and/or after the cartel. As pointed out by Connor (2007),

" this method should be called the " with-and-without collusion method" since the " before" period is really any nonconspiracy period---whether before, after, or during an intermediate pause in price-fixing."

This method does not control for shifts in demand or cost functions. For that reason, Connor (2010b states:*"it is important that the " before" period be one that is quite comparable to the conspiracy period with respect to demand and supply conditions. Shifts in buyer preferences, appearance or the disappearance of substitutes, or changes in the cost of production of the cartelized product during the affected period can cause overstatement or understatement of the overcharge."*

Figure1

Before-and-After Methods



The lack of robustness of the before-and-after method is illustrated by the following citation from Finkelstein and Levenback (1983):

"An obvious idea is to assume that competitive prices during the conspiracy period would have been the same as they were before or after the conspiracy or in interludes of competition within the conspiracy period [...] This estimate, however, meets the immediate objection that it is likely to be incorrect because changes in factors affecting price other than the conspiracy would have produced changes in competitive prices if there had been competition during the conspiracy period."

This lack of robustness is further enhanced by the difficulty of correctly specifying when the conspiracy begins and when it ends. Indeed, an incorrect specification of the conspiracy period may result in biased estimates and lead to wrong conclusions, as illustrated by the following citation from Levenstein, and Suslow (2002):

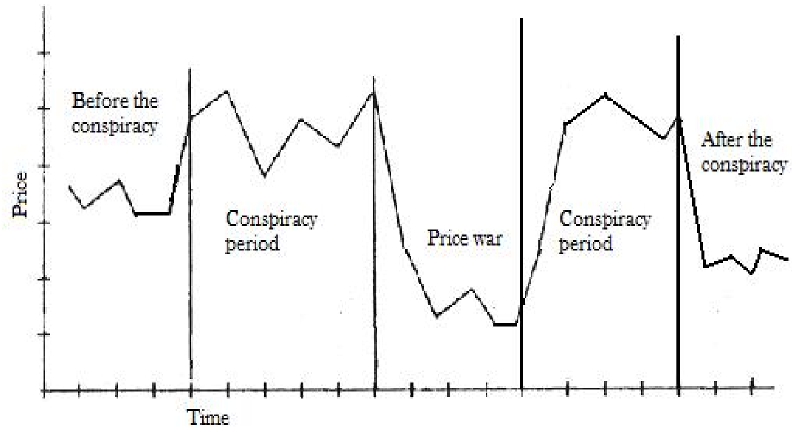
"Connor writes that there was " disagreement about the dates of the conspiracy-effects period, the but-for price, and the type of industry conduct absent collusion... " Connor uses marginal cost (estimated from what he identifies as " highly competitive" periods) as the competitive price."

2.2 The " Price during a price war" Method

This method uses the price during a price war of laps of collusion to proxy the but-for price. This method is basically an instance of the before-and-after method and thus suffers from the same limitations.

Figure 2

Price war interlude



In Yinne (2003), the *"overcharge is calculated as imports*price increase/(1+price increase), and the price increase is estimated from the observed price drop subsequent to the cartel's demise."* Unfortunately, the price drop that is referred to in this citation is probably not driven by normal economic forces. In general, prices during a price war can be significantly lower than in natural competition equilibrium. As acknowledged by Connor (2007), *"a predatory episode before cartel formation will strongly overestimate the overcharge, as happened in lysine."*

2.3 The Yardstick Method

This method compares the prices during the conspiracy period with comparable or yardstick, assumed competitive firms, product or geographic markets during the alleged cartel period. The yardstick method should be used with caution because an increase in price due to domestic market cartelization can cause a partial demand shift toward nearby markets. Similar domestic firms that are not participating in the collusion will tend to follow the cartel price (umbrella effect). Connor (2010b) states:

"The yardstick approach involves the identification of a market similar to the one in which prices were fixed but where prices were unaffected by the conspiracy. A yardstick market should have cost structures and demand characteristics highly comparable to the cartelized market, yet lie outside the orbit of the cartel's influence."

Hence, the Yardstick method may not be appropriate in the case of certain international cartels (e.g. the ADM-lysine case).

2.4. The " Cost-Based" Method

This method is based on the observation that changes in price should reflect changes in costs. The direct way to apply this method is to estimate the production costs by using the (accounting) information on firms involved in the cartel. In the lysine cartel case, prosecutors have introduced the confidential production and sales records of ADM as exhibits, and these documents are now publicly available. (See Connor 2001).

But typically, academic researchers and economic experts do not have access to confidential documents of firms. In general, the overcharge is thus approximated by substracting a *" reasonable margin"* from the actual cartel profit and dividing by the production volume. The reasonable margin should include not only the marginal production cost, but also other factors that causes the natural competition price to be larger than the marginal production cost. In particular, it should include opportunity costs, the risk premium and oligopoly mark-up when relevant. Failure to account for these would lead to overestimating the overcharge.

2.5 "Econometric" Methods

Econometric methods are not tied to a particular economic theory. This denomination gathers all methods using more or less sophisticated econometric models to assess the but-for price. Econometric methods can be used to simulate an oligopolistic competition (Cournot, Bertrand), to predict the Lerner index of market power or to estimate a demand and cost functions that account for dynamic market conditions. A simple example is given in Froeb, Koyak and Werden (1993): *"To estimate conspiracy-free prices for the earlier periods, we first fit logarithms of frozen perch winning bid prices for the post-conspiracy period on the logarithms of fresh perch prices for the corresponding month and for the prior five months. These opportunity cost variables explain 77.0% of the variance in frozen perch bid prices. This regression is then used to `backcast' predicted, conspiracy-free prices for the earlier periods."*

The potential of this method is illustrated by the following citation from Connor (2007): *"Demand for animal feed rises in the winter months, which results in an increase in the derived demand for lysine in the fall of each year. Econometric methods are better equipped to handle seasonal shifters than the simple before-and-after method. Because collusion is best timed to begin when seasonal demand rises, ignoring this factor will lead to an overestimate of damages".* This citation suggests that an econometric model if correctly built is more robust than simpler methods. However, this does not necessarily mean that econometric methods are always more reliable than other methods. In fact, building a good econometric model requires a careful selection of the relevant control variables to include. Also, the estimation results must be interpreted in light of the theory underlying the model.

3. The Characteristics of the Connor Database

In this section, we review some important characteristics of the Connor sample that one should be aware of before analyzing the data.

3.1. The data are estimates rather than natural observations

Economists devote time and resources to design overcharge estimation methods because cartels are usually not willing to communicate the true amount of their illegal profits. Due to this, overcharges are typically estimated rather than observed. The Connor sample consists of estimates previously computed and published by different analysts and researchers, and is therefore subject to model errors, estimation errors and sample selection.

In fact, overcharge estimators typically build on existing economic theories or models. As any other estimator, overcharge estimators are subject to model error due to the fact that a model is necessarily a simplified representation of the world. Model errors often translate into misleading choice of estimation method, and hence creates estimation biases. An estimator is said to be robust to model error if it continues to measure with precision the parameter of interest when the model that has actually generated the data deviates to some extent from the theory used to derive the estimator. The robustness of an estimator to model error can be studied by making specific assumptions about the true data generating process. For example, linear regression model are estimated by OLS based on the assumption that the error term is independent of the regressors. To verify the robustness of the OLS estimators to this assumption, one may postulate a particular form of dependence between the error and the regressors, and then, study the properties of the previous estimators under this alternative assumption (See Hayashi (2000), page 188). As another example, suppose we want to determine the equilibrium mark-ups on a given market. The assumptions made about the type of interaction in which the players are involved strongly determines the nature of the equilibrium. If one assumes that the competition is pure and perfect, then the equilibrium mark-ups would be null. On the other hand, if one assumed an oligopolistic or monopolistic competition, then the mark-ups are allowed to be positive at equilibrium.

To understand the estimation error as opposed to model error, suppose in the example above that the market model is correctly specified and the theoretical formula of the mark-up correctly derived within this model. To implement the model empirically, one needs to replace the unknown parameters involved in the mark-up formula by their random estimates. This causes the final mark-up estimator to be itself random. The difference between the mark-up estimate and the true unknown mark-up is an estimation error. This error does not vanish even if the model is correctly specified. For example, an estimator of the theoretical variance of a distribution is given by the empirical variance of a random sample from this distribution. In this case, the estimation error may be defined as the root mean square error of the empirical variance.

The sample selection refers to the extent to which the sample available for estimation is representative of the true population. A sample selection problem arises if observations that meet certain criteria are absent from the sample. In the Connor database, the sample selection problem takes the form of the publication bias. This type of selection stems from the fact that "…*editorial reviewers have a substantial preference for studies with statistically significant results*" (Hunter and Schmidt, 2004). The potential distortions that the publication bias can cause to observed sample of cartel overcharges is pointed out by the following citation from Ehmer and Rosati (2009): "*If a researcher finds that a cartel had the effect of strongly increasing prices, the result is likely to be considered correct and worth publishing. However, if the analysis shows that the cartel had the effect of reducing prices, the conclusion is more likely to be discarded as unreliable, and the result not published*". In the Connor sample, the publication bias does not take the form of a systematic exclusion of zero overcharge estimates as they represent 7.23% of the sample. Rather, one can only suspect that the proportion of zeros would be higher if the actual overcharges were observed. Moreover, some of the zero estimates may be negative estimates that are rounded up to zero.

In his answer to Ehmer and Rosati published in the same journal, Connor (2010a) mentions another type of sample selection: "*It has long been believed that discovered cartels are more inept than cartels that remained hidden from antitrust authorities. This is what is meant by selection bias in cartel studies. And this kind of selection bias suggests that the overcharges of discovered cartels are systematically lower than overcharges of secret cartels*". This assertion of Connor is highly debatable. Indeed, if the probability of detection of a cartel is strictely positive, then a cartel cannot remain undiscovered forever. Since a cartel can be dissolved voluntarily or be victim of deviation by its members before being discovered, there is no compelling reason to expect that undiscovered cartels live longer than those discovered, and hence, are more likely of making larger cumulative profit. Moreover there is no reason a priori to suggest that long-lived cartels have higher *yearly profits* than short-lived ones.

In summary, a sound treatement of potential model errors, estimation errors and sample selection is necessary in an empirical analysis of the Connor sample.

3.2. Skewness, outliers and heterogeneity

In the abstract of Connor (2010b), one reads: *"the median long run overcharge for all types of cartels over all periods is 23.3%. [...] Cartel overcharges are negatively skewed, pushing the mean overcharge for all successful cartels to 50.4%"*. Actually, Connor means to say that the overcharges are positively skewed, a large proportion of small overcharges coexisting with a small proportion of large overcharges. This small number of large overcharge estimates are influential observations that cause the mean computed on the whole sample not to be representative of the majority of cartels. In the Connor sample used in this paper, the average and median of the sample are respectively 45.5% and 23% for all cartels and 49.0% and 25.0% for cartels with stricly positive estimates. The difference between the mean and the median is due to the skewness of the distribution of the overcharge estimates.

Figures 3 and 4 confirm that in the Connor sample, the large magnitude of the empirical mean is due to a few number of influential. Roughly 1% of overcharge estimates are larger than 400% and 22.6% larger than or equal to 50%. Such overcharge levels would be quasi impossible under normal economic conditions. This suggests that these outliers should be treated carefully when using econometric methods that are sensitive to their presence. For example, it is well known that when using ordinary least squares, the presence of outliers in the sample significantly increases the likelihood of a misleading conclusion.

Figure 3

Overcharge estimates: Distribution skewed to the right.
Overcharges larger than 400% (1% of the sample) are not shown on this figure.

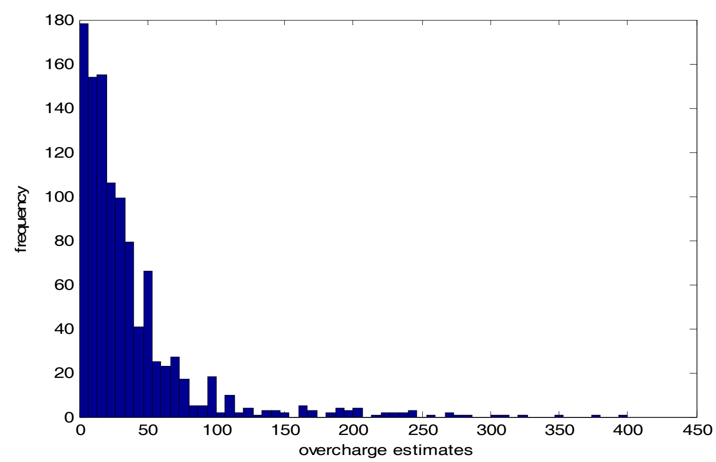
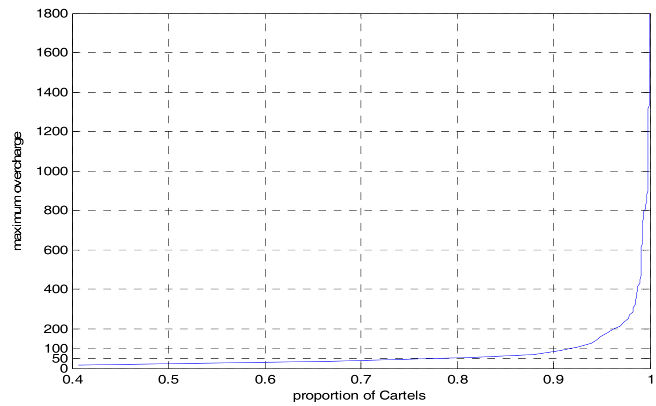


Figure 4

Proportion of cartels with overcharge estimates smaller than y, where y is given by the vertical axis.



US cartels with overcharge estimates larger than or equal to 50% represent 5.7% of the sample, while such EU cartels represent 8.8% of the sample. The mean overcharge estimate for these subsamples are respectively 126.1% and 113.7%. Also, the means for US and EU cartels with stricly positive overcharges lower than 50% are 19.7% and 19.2% respectively. Overall, the average positive overcharges are 42.0% for US cartels and 45,6% for EU cartels.

A similar picture can be drawn by comparing older cartels (ante 1973) to more recent ones (post 1973). Indeed, the average overcharge estimates are 62.0%, 47.8% and 43.8% respectively for all cartels, US cartels and EU cartels before 1973, as compared to 38.9%, 33.6% and 41.9% respectively for more recent cartels. Although the overcharge estimates are potentially biased, they suggest that cartels may behave differently across countries and periods. See Table 1.

Another source of heterogeneity one can think of is the domestic versus international criterion. Domestic cartels represent 46.79% of all cartels and have average overcharge estimate equal to 33.60%. On the other hand, the average overcharge estimate of international cartels is equal to 55.90%, which is about 12% higher than for domestic cartels. International cartels with overcharge estimates higher than 50% represent 16.25% of the sample. This means that international cartels represent more than two third of the category of cartels with overcharge estimate higher than 50%. The average overcharge estimate of international cartel is 135.68% on the subsample with estimates larger than 50%. However, this average is 5.63% higher for the corresponding domestic cartels.

Table 1

Mean and median overcharge estimates (OE) per location and types of cartels.
The prop.% are fractions of the total Connor sample (1120 cartels).

		All Cartels	OE>0%	0%<OE<50%	OE≥50%	Cartels Before 1973	Cartels After 1973
All locations	Mean	45.47	49.01	**20.61**	**137.26**	**61.98**	**38.89**
	Median	*23.00*	*25.00*	*18.38*	*74.00*	*29.00*	*21.40*
	prop.	100.00	92.77	70.18	22.59	28.50	71.50
US	Mean	**38.15**	42.03	19.69	126.14	47.79	33.58
	Median	*20.50*	*23.50*	*17.50*	*70.20*	*30.50*	*16.80*
	prop.	30.00	27.23	21.52	5.71	9.64	20.36
EU	Mean	**42.65**	45.57	19.19	113.65	43.83	41.86
	Median	*23.00*	*25.00*	*16.10*	*75.00*	*24.75*	*20.40*
	prop.	33.48	31.34	22.59	8.75	13.39	20.09
Domestic	Mean	33.60	36.91	18.66	141.31	35.42	32.79
	Median	***17.05***	*19.00*	*16.45*	*71.00*	*20.50*	*16.45*
	prop.	46.79	42.59	36.25	6.34	14.46	32.32
International	Mean	**55.90**	59.28	22.70	135.68	89.38	43.93
	Median	*30.00*	*31.88*	*22.00*	*74.45*	*37.00*	*27.50*
	prop.	53.21	50.18	33.93	16.25	14.02	39.20

The heterogenous nature of the Connor sample raises serious agregation problems. In concrete terms, the average overcharge obtained for the whole sample is meaningful only if the conditions that determine the but-for price are the same across time and markets. These conditions are obviously subject to change, and we can therefore expect significant aggregation biases. The following citation from Levenstein and Suslow (2003) illustrates the danger of using one single number to summarize all overcharge data: *"The reported price increases vary widely by industry and by source. At the low end, for example, we have a reported price increase of ten percent for the thermal fax paper cartel, which was formed as the industry was declining and lasted for less than a year. At the high end there is the stainless steel cartel, which reportedly almost doubled prices. This cartel lasted slightly more than one year (from January 1994 to March 1995) and involved six European steel companies."*

Thus, a carefull treatement of outliers and heterogeneity is necessary in empirical analyses of the Connor database.

3.3. Reliability of the Estimation Methods

In studying the overcharge, it is important to bear in mind that the observed time series of prices are resultant of several causes. For example, an inelastic demand may grant a firm with a significant market power that translate into high mark-ups. Homogenous products may be perceived as differentiated by consumers due to the positioning of the product by the firm's advertisement policy. The product differentiation in turn can cause a previously pure competitive market to behave as an oligopolistic or a monopolistically competitive one. But typically, oligopolistic markets have margins over *MC* that are significantly above zero, as illustrated by the following citation from Morrison (1990): *"The empirical results suggest that mark-ups in most U.S. manufacturing firms have increased over time, and tend to be countercyclical."* In Morrison (1990), one further reads*:"...Robert Hall [ref.], [...] reported both significant increasing returns and mark-ups of price over marginal cost in various U.S. industries. Hall also finds that economic profits are approximately normal, suggesting an industrial structure along the classic lines of monopolistic competition."*

The proper but-for price should thus be the one that characterizes the natural competition equilibrium for the targeted market. More precisely, the but-for price is equal to the marginal cost plus a margin over marginal cost. In pure and perfect competition, this margin is low and close to zero. Regarding the complexity of the accurate characterization of the but-for world, some methods that have been used in the literature to estimate the overcharge are strikingly simplistic (and probably grossly inadequate). Examples include:

- Bolotova, Yuliya, John M. Connor, and Douglas Miller. The impact of Collusion on Price Behavior: Empirical Results from Two Recent Cases, paper at the 3rd International Industrial Organization Conference, Atlanta, April 9, 2005.
  "We used extensions of traditional ARCH and GARCH models to examine the difference in the behavior of the first two moments of the price distribution during collusion and the absence of it using prices from two recently discovered conspiracies, citric acid and lysine. According to our results, the citric acid conspiracy increased prices by 9 cents per pound relative to pre-cartel and post-cartel periods. The lysine conspiracy managed to raise prices by 25 cents per pound."

  However, reduced form models like ARCH and GARCH are designed to capture variations over time of the mean and variance of a stochastic process without necessarily explaining what determines these variations. For such models to be fully relevant for the analysis of cartel overcharges, they must explicitly control for other determinants like changes in supply and demand functions, cost functions, seasonalities in the price process, etc.

- John M. Connor. The Global Lysine Price-Fixing Conspiracy of 1992-1995. Review of Agricultural Economics, Vol. 19, No. 2 (Autumn - Winter, 1997), pp. 412-427
  "But-for price is average of the time series of price over two non-conspiracy periods."

Caveat: Same drawbacks as the before-and-after method. This approach assumes that nothing else than a price-fixing conspiracy may generate an upward drift in prices.

- John M. Kuhlman. Theoretical Issues in the Estimation of Damages in a Private Antitrust Action. Southern Economic Journal, Vol. 33, No. 4 (Apr., 1967), pp. 548-558.

  The author used the maximum of the price index before the conspiracy as but-for-price. The author argues that at worst, this approach will underestimate the overcharge. Unfortunately, it has the same drawbacks as the before-and-after method. The maximum price observed in the past may not be informative about the new state of the world.

- Bosch, J. and E.W. Eckard. The Profitability of Price Fixing: Evidence from Stock-Market Reactions to Federal Indictments. Review of Economics and Statistics 73 (1991): 309-317.

  The authors assessed the monopoly profit losses from the market reaction to indictment announcements. They found the market reaction to be significant. However, what is measured by the authors is the market's assessment of the decrease in future payoffs following the cartel detection, which can be quite different from the actual overcharge.[4]

- In the ADM-lysine case, the approach advocated by Connor in estimating the overcharge as well as what he considers as the effective conspiracy period remains an area of disagreement.

  For example, White (2001) wrote:*"Connor [...] has claimed that the trebled damages to lysine purchasers were an order of magnitude larger [than the 45 million settlement proposed by ADM]. Crucial to Connor's conclusions are his assumptions as to the time period during which the conspiracy had an effect on prices and the but-for price that otherwise would have prevailed in the absence of the conspiracy. This paper will argue that Connor substantially over-estimated the period of the conspiracy and under-estimated the but-for price."*

  In the following citation from Levenstein and Suslow (2002), White further expressed his doubts about the quality of the estimates of Connor:*"White [...] argues that the lysine industry, absent cooperation, would not have operated as a perfectly competitive industry. It was a four-firm oligopoly that would have likely been able to engage in some form of implicit coordination if there had been no explicit meetings. Recognition of this possibility, White argues, causes us to " enter the world of oligopoly speculation." Perhaps ADM*

[4] Some investors may be valuing at a premium the stock of a company condemned for being involved in cartel conspiracy if their assessment of future expected profits more than compensate current losses.

would have operated as a dominant firm. Or, perhaps the firms would have adopted Cournot behavior. [...] He concludes that " though the conspiracy surely did have harmful effects on the purchasers of lysine, those effects were less extensive and less severe than was claimed."

Connor (2000, Appendix Table A3) estimates the overcharge for the Lysine cartel (1992-1995) using the before-and-after approach. Even accounting for seasonal shifts of the demand curve, his lowest estimate is $157.8 million dollars. Later on, he acknowledges that his estimates are exaggerated: *"With the benefit of hindsight and a great deal more information, it appears now that the first $150 million estimate by the plaintiffs* [Connor (2002)] *was too high."*

The revision by Connor of his own estimates appears to be due to the influence of competing empirical results, especially Morse and Hyde (2000) who developed and tested a richly specified econometric model of the lysine industry using 1990-1995 monthly data. After estimating their model, Morse and Hyde found that the lysine cartel's overcharge was $71 million. Using a before-and-after method, Connor finally arrived at an estimate of about $80 million by revising his but-for price from $0.70 to $0.80. Connor (2002) wrote the following comment about the methodology of Morse and Hyde: *"Morse and Hyde (2000) managed to incorporate a fairly complete list of determinants of lysine demand: the number of hogs needed by U.S. slaughter houses, red meat and poultry export demand, the price of a complement and a substitute (the shadow price discussed above), and seasonality of lysine demand. On the supply side, an equation related ADM's U.S. production to the cost of three principal inputs: dextrose, other variable costs of manufacture, and capital. Both of these equations fitted the five years of data quite well, and the signs were the ones predicted by economic reasoning. Finally, an innovative feature of the model is an equation that permits the researcher to measure the degree of competitiveness ("conjectural variations") between ADM and its four rivals."*

In the following citation, Connor (1998) acknowledged that the accurate measurement of the overcharge is a complex task that requires the simultaneous control of several factors: *"Overcharge estimates are sensitive to a number of assumptions, most notably the " but-for" price---the market price that would have been observed had there been no cartel. Perhaps $0.60/lb. is too low a but-for price. [...]. However, about 18 months after the conspiracy ended, spot transactions prices had drifted only down to $0.67 or $0.68/lb., so the post-cartel period prices may hint that production costs had risen or that tacit collusion was being observed in 1997. Thus, the but-for price could have been $0.64/lb to $0.68/lb. At $0.68, the overcharges would be reduced to a bit over half of the estimates made previously."*

In the next section, we review some problems that are not tied to the reliability of the computation methods, but instead to wrong interpretations of possibly well computed estimates.

3.4. Misinterpretation of previous results

The mark-up estimates of Morrison (1990, 1993) are converted into overcharges in Connor (2010b) while Morrison herself attributed these mark-ups to monopolistic competition. Unfortunately, these are not the only results from previous studies that have been wrongly interpreted as cartel overcharges by Connor.

For example, Bhuhan and Lopez (1997) estimated a system of demand, from which estimates of Lerner indices are deduced. They found firms operating in the food and tobacco industries to have oligopolistic market powers. They never claimed that their results provided any kind of proof that the industry was cartelized. However, the Lerner indices estimates of Bhuhan and Lopez have been converted into overcharges in Connor (2010b).

Barnett et al. (1995) estimated a model of oligopoly price behavior and studied the impact of US state level and federal taxes on cigarette consumption. They estimated various elasticities and conjectural variations than can be used to approximate oligopoly mark-ups. They never claimed that these mark-ups was due to overt collusion. However, the estimates of Barnett et al. have been converted into overcharges in Connor (2010b).

Suslow (1986) specified and estimated an econometric model of the aluminum industry between World War I and World War II. The study concluded that Alcoa had a substantial market power. According to the author: *"Over the sample period Alcoa was not directly involved in the cartel, but there seemed to be implicit reciprocal agreements between Alcoa and the cartel against exporting into each other's territory".* Connor (2010) is much more affirmative (in appendix): *"[Suslow (1986) is a study] that measures the U.S. market power of Alcoa during three episodes when it was a monopolist in the U.S. market (1923-1940) partly because of agreements with European producers that limited imports"* .

Christie and Schultz (1994) is a study aimed at explaining the lack of odd-eighth quotes for the majority of NASDAQ stocks and raised the question as to whether NASDAQ dealers implicitly collude to maintain wide bid-ask spreads. They observed that odd-eighth quotes represented close to 50% of the price fractions for 5 NASDAQ firms whose market makers ceased using odd eighths in 1991. More importantly, they concluded that their "*data do not provide direct evidence of tacit collusion among NASDAQ market makers*". In reference to this paper, Connor (2010b) reported an overcharge of 50%.

Runar (1989) observed that the Swedish pulpwood market is one of imperfect competition that can be modeled as a monopsony. He then undertook a study to estimate the effects on prices and quantities by going from an imperfect to a competitive pulpwood market. Connor (2010b) reported overcharges of 10% and 25% for the Swedish pulpwood and sawtimber markets respectively. However, the author reported results of a loss of approximately 23% of the value of all pulpwood sales and 10% of the value of total roundwood sales and further stated that he could not refute his assumption of monopsony in the pulpwood market.

The list of shortfalls drawn up above is non exhaustive. Furthermore, a significant proportion of the overcharge estimates used in Connor (2010b) are obtained from the conversion of Lerner indices into overcharges. Below, we illustrate the danger of converting a Lerner index into an overcharge estimate by ignoring the presence of competitive mark-ups.

3.5. Misuses of the Lerner Index

An overcharge calculation approach based on the Lerner index can fall within the family of econometric methods or cost-based methods depending on how this index is estimated. The Lerner index of market power is defined as:

$$L = \frac{p-c}{p} \tag{3}$$

where p is the market price and c is the marginal cost (MC). In a perfectly competitive market, $p = c$ so that $L = 0$.

If the condition that would prevail in the absence of cartels is perfect competition, then the but-for price is given by $p = c$. The Lerner index for the cartelized market is then given by:

$$L = \frac{\tilde{p}-c}{\tilde{p}} \tag{4}$$

and the true overcharge is:

$$\delta = \frac{\tilde{p}-p}{p} = \frac{\tilde{p}-c}{c} \Leftrightarrow \delta = \frac{L}{1-L} \tag{5}$$

where $p = c$ is the but-for price in this case. Hence the formula above permits to retrieve the overcharge from the Lerner index if the but-for world is assumed to be characterized by pure and perfect competition.

In a natural competition (real life) context, the price is in general equal to the MC (c) plus a margin over MC (m). The but-for price is then given by $p = c + m$. The Lerner index in the cartelized market is:

$$L \equiv \frac{\tilde{p}-c}{\tilde{p}} = \frac{\tilde{m}}{c+\tilde{m}} \tag{6}$$

where $\tilde{p} = c + \tilde{m}$ is the price in the cartelized market. Hence the (true) overcharge by the cartel over the natural competition price becomes:

$$\delta \equiv \frac{\tilde{p}-p}{p} = \frac{\tilde{m}-m}{c+m} \tag{7}$$

However, the overcharge that would be inferred from the Lerner index (wrongly) assuming perfect competition as benchmark is:

$$\tilde{\delta} \equiv \frac{L}{1-L} = \frac{\tilde{m}}{c} = \delta + \frac{m}{c}(\delta + 1) \tag{8}$$

Typically, oligopolistic competition mark-ups range between 10% and 30%, and sometimes more (Morrison 1990, 1993). If the true overcharge is $\delta = 10\%$ and $\frac{m}{c} = 20\%$, then the Lerner index delivers the biased estimate $\tilde{\delta} = 32\%$, i.e. more than three times the true value. Note that the bias is increasing in both the true δ and the natural competition mark-up to marginal cost ratio $\frac{m}{c}$. For example, $\delta = 20\%$ and $\frac{m}{c} = 20\%$ leads to the biased estimate $\tilde{\delta} = 44\%$. Now assume that the true overcharge is half of previous one ($\delta = 10\%$) and that $\frac{m}{c} = 30\%$. Then the overcharge inferred from the Lerner index is $\tilde{\delta} = 43\%$, i.e. almost the same value as in the previous example where the true overcharge is higher.

If the unit cost c is small, the bias $\frac{m}{c}(\delta + 1)$ will tend to be large, no matter how small the true δ is. This is illustrated by the following table.

Table 2

Pitfall in the Conversion a Lerner Index Into an Overcharge Estimate.
Constant overcharge, constant margin, and bias increasing as the marginal cost decreases.

Parameters	Values				
δ	10%	10%	10%	10%	10%
m	0.02	0.02	0.02	0.02	0.02
c	0.20	0.1625	0.1250	0.0875	0.05
$\frac{m}{c}$	10%	12.3 %	16%	22.9%	40%
Bias $= \frac{m}{c}(\delta + 1)$	11%	14%	18%	25%	44%
$\tilde{\delta} = \delta + Bias$	21%	24%	28%	35%	54%
$\frac{\text{Bias}}{\delta}$	52.4%	58.3%	64.3%	71.4%	81.5%

In Connor (2006, Table 12, 12A, 12B), the overcharges are inferred from Lerner indices previously estimated by Bernheim (2002).[5] The average overcharge inferred by Connor is above 40%, a high percentage potentially attributable to the propensity of the calculation method used to overstate the true illegal profit. In the following citation, Connor (2007) seems to acknowledge that the but-for price should reflect the true conditions prevailing before the cartel formation: *"A pre-cartel price is often presumed in legal settings to be the competitive price. " Cartel members . . . enjoy no presumption that they already had market power before the illegal act was committed" (Hovenkamp, 1998:660). However, even if a pre-cartel period was arguably one of oligopolistic tacit pricing conduct, the pre-cartel price is still a reasonable benchmark so long as the competitive determinants of pricing conduct did not change when the cartel was formed."* Also, in Footnote 3, page 5 of Connor (2010b) it is mentioned that: *"...The benchmark may be the purely competitive price, or it may be a somewhat higher price generated by legal tacit collusion by companies in an oligopolistic industry."* However, Footnote 47 on page 16 casts some doubt on how he actually interprets the overcharges estimated from the Lerner index in subsequent analyses: *"...The Lerner Index is the same as the overcharge, except that it is measured by dividing [the difference between] the market price [and the marginal cost] by the monopoly price instead of the competitive price".*

We have illustrated the bias tied to methodologies based on the Lerner index. But each estimation methodology is susceptible of introducing a particular type of bias. One way to size up the bias contaminating overcharge estimates is to build an econometric meta-analysis model of overcharges. The goals of such a meta-analysis and the relevant models to use are discussed in the next section.

4. A Meta-Analysis to Bias-Correct Overcharge Estimates

As explained in Connor and Bolotova (2006), a meta-analysis may be defined as an "analysis of analyses". It is often used in experimental fields to summarize the findings of studies in a particular literature. But increasingly, meta-analyses are used to verify if the conditions of an experiment impact the results. As pointed out by Frank Schmidt in the preface to the second edition of his book coauthored with John Hunter, "*The avowed purpose of other methods of meta-analysis is to describe and summarize the results of studies in a research literature (Rubin, 1990). [...] In our view, the purpose of meta-analysis is to estimate what the results would have been had all the studies been conducted without methodological limitations and flaws*". The meta-analysis conducted in this section is consistent with the view of Hunter and Schmidt (2004).

[5] As quoted by Connor. See also Kovacic et alii (2005).

In their meta-analysis, Connor and Bolotova (2006) used a linear regression to analyze the relationship between cartel overcharge estimates on one hand and the characteristics of cartels, the estimation methods and publication sources of the estimates on the other hand. They found that the characteristics of the cartel explain the variability of overcharge estimates to a greater extent than the estimation methods and sources of publication. We argue that this analysis is incomplete, as it fails to point out that the portion of overcharge estimates that is "explained" by subjective factors like the estimation method or the source of publication is an estimation bias. Moreover, outliers are included in their regression analysis that cause their result to lack robustness. These points are made clearer below.

4.1. True Overcharge vs. Estimation Bias

It is reasonable to expect that the true overcharge values depend on the conspiracy period, the duration of the cartel, the characteristics of the firm involved in the collusion and other similar factors. However, we do not observe the true overcharge. Instead, we observe an estimate which is equal to the actual overcharge plus a bias, positive or negative. Hence in addition to factors that affect the true overcharge, we can arguably expect the estimate to be sensitive to subjective factors that may cause bias, namely the estimation method, the source of publication and other factors "posterior" to the occurrence of the conspiracy period.

Formally, the bias is defined as the influence of factors that affect the overcharge estimates, but not the true overcharge values themselves. Indeed, the true overcharge is already empocketed by the cartel by the time the estimation is being made. Hence the true overcharge should not depend on the estimation method nor on the source of publication. To bias-correct the estimates, one can build and estimate a meta-analysis model that relates overcharge estimates to both types of factors. Examples of variables capable of explaining the size of actual overcharges include the duration of the cartel, its organizational characteristics, its scope (domestic vs. international), the conspiracy period, the industry characteristics (sector, concentration, etc), the elasticity of demand, etc. On the other hand, example of variables that may explain the size of the estimation bias are the estimation method, the publication source, the presumption of collusion when computing the estimator, etc.

Unfortunately, the data set available for our analysis does not include variables that describe the industry characteristics or the elasticity of demand. The type of analysis conducted below could thus be improved if and when more data on cartels become available.[6]

[6] If measures of market concentration were available, these could have been used as explanatory variables although not directly convertible into cartel overcharge.

4.2. A Linear Meta-Regression Analysis of Cartel Overcharges

By definition, the true overcharge θ_i depends only on variables Y that impact its size:

$$\theta_i = \alpha + Y_i\phi + u_i \tag{9}$$

where u_i is an error term such that $E(u_i) = E(u_i Y_i) = 0$.

We have seen previously that converting a Lerner index into an overcharge generates a multiplicative bias. Hence in general, we can expect the estimated overcharge to take the following form:

$$X_i = (1 + Z_i\lambda)\theta_i \tag{10}$$

where X_i is the overcharge estimate from study i, Z_i is the set of variables that explain the size of the bias. This amounts to say that the overcharge is inflated or deflated by a factor $Z_i\lambda$, i.e. a linear combination of the Z variables. By substituting for θ_i into the expression of X_i, we obtain:

$$X_i = \alpha + Y_i\beta + Z_i\gamma + Y_i Z_i\delta + \varepsilon_i \tag{11}$$

where:

$$\beta = \alpha\phi,$$

$$\gamma = \alpha\lambda,$$

$$\delta = \lambda\phi,$$

$$\varepsilon_i = (Z_i\lambda + 1)u_i,$$

$Y_i Z_i$ is the set of interaction variables and ε_i is an error term. This suggests that ε_i is heteroscedastic, but we ignore this in the sequel.[7]

Consider, for example, the interaction variable obtained by multiplying the duration by the yardstick dummy variable. The estimated coefficient for that variable gives the additional bias induced by the yardstick method per unit duration. Also, the coefficient of the interaction between the yardstick dummy variable and the dummy of a given cartel characteristic (e.g: domestic vs. international) gives the additional bias of the yardstick method when used to estimate the overcharge of cartels with this particular characteristic. But note that the indicator variable obtained by interacting two dummies can be identically null (if the intersection between the two subsamples

[7] When heteroscedasticity is ignored, the coefficients estimated by OLS are consistent, but their standard error tends to be underestimated. At worst, this may lead to conclude that a coefficient is significant while it is actually not. In our analysis, all estimated coefficients are used to bias-correct the overcharge estimates, regardless of whether they are significant or not.

identified by these dummies is empty) or identical to an existing variable. Such interaction variables are not relevant and must be discarded. Furthermore, if both the number of Y and Z variables are moderately large, this results in a huge number of interaction variables that cause the model to lack parsimony.

To mitigate these difficulties, we use the indicators of clusters identified in the previous section as a summary of the Y variables, and only these indicators are interacted with the Z variables. The model we estimate is:

$$\mathrm{X_i} = \alpha + \mathrm{Y_i}\beta + \sum_{k=1}^{4} \mathrm{Z}_{.,i}^{(k)}\gamma_k + \varepsilon_i \tag{12}$$

where $\mathrm{Z}_{.,i}^{(k)} = \left(Z_{1,i}^{(k)}, \dots, Z_{K,i}^{(k)}\right)$, $Z_{j,i}^{(k)}$ being the realization of the interaction variable $Z_j^{(k)}$ for cartel i. This interaction variable is obtained by multiplying the variable Z_j by the indicator of cluster *k*. Hence $Z_{j,i}^{(k)} = Z_{j,i}$ if the cartel i belongs to cluster k, and $Z_{j,i}^{(k)} = 0$ otherwise. Note that this amounts to assuming that the parameters of the bias-correction factors vary across clusters.

4.3. A Log-linear Meta-Regression Analysis of Cartel Overcharges

Alternatively to the linear model (12), we can also consider the log-linear specification:

$$\log\mathrm{X_i} = \alpha + \mathrm{Y_i}\beta + \sum_{k=1}^{4} \mathrm{Z}_i^{(k)}\gamma_k + \varepsilon_i \tag{13}$$

This model implies that:

$$\mathrm{X_i} = \exp(\alpha + \mathrm{Y_i}\beta + \varepsilon_i)\exp\left(\sum_{k=1}^{4} \mathrm{Z}_i^{(k)}\gamma_k\right) \tag{14}$$

We see that if the true overcharge is given by $\theta_i = \exp(\alpha + Y_i\beta + \varepsilon_i)$, then the bias is multiplicative and given by:

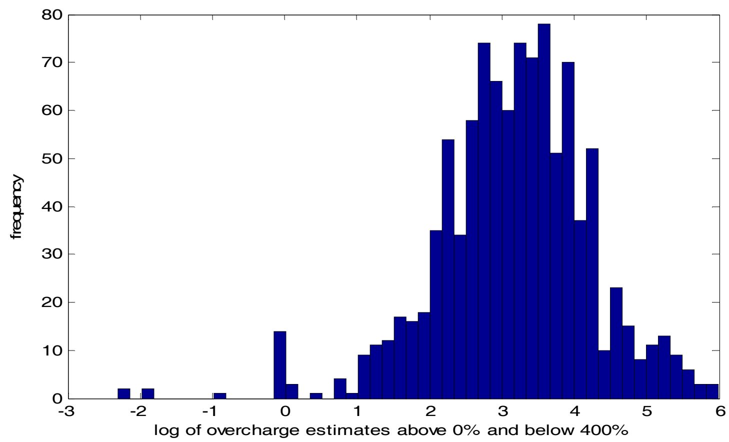
$$\begin{aligned} \mathrm{Bias}(\theta_i) &\equiv \mathrm{X_i} - \theta_i \\ &= \exp(\alpha + \mathrm{Y_i}\beta + \varepsilon_i)\left[\exp\left(\sum_{k=1}^{4} \mathrm{Z}_i^{(k)}\gamma_k\right) - 1\right] \end{aligned} \tag{15}$$

This formula allows the bias to be positive, null or negative depending on whether $\exp\left(\sum_{k=1}^{4} Z_i^{(k)}\gamma_k\right)$ is larger than, equal to or smaller than unity.

Another argument in favor of the log-linear specification is that the distribution of the logarithm of estimated overcharges is close to symmetry and less subject to distorsion caused by extreme values, as showed in the following figure. This happens because the log-transformation shifts part of the right skewness into left skewness. In the sequel, the estimation results of the two model specifications are compared.

Figure 7

Log of overcharge estimates that are above zero and below 400% (91.6% of the sample).



4.4. An Example of Meta-Analysis: Connor and Bolotova (2006)

Connor and Bolotova (2006) performed a meta-analysis of cartel overcharge estimates in which they used the following variables:

Y_1: Duration takes value 1 if duration is less than 5 years; 2 if duration is from 6 to 10 years; 3 if duration is from 11 to 15 years; 4 if duration iis 16 years or more.

Y_2: Domestic cartel: 1 = Yes; 0 = No.

Y_3: Bid rigging: 1 = Yes; 0 = No.

Y_4: Found guilty or pleads guilty: 1 = yes; 0 = No.

Y_5: Geographic market (dummies for US, EU, ASIA, ROW including Latin America, WORLD cartels which cannot be associated to a head region)

Y_6: Antitrust law regime (dummies for P1: 1770-1890, P2: 1891-1919, P3: 1920-1945, P4: 1945-1973, P5: 1974-1990, P6: 1991-2004).

Z_1: Overcharge estimation method (dummies for *Price before conspiracy, Price war, Price after conspiracy, Yardstick, Cost based or normal profit, Econometric modeling, Historical case study with no method specified, Other*)

Z_2: Type of publication (dummies for *Peer reviewed journal, Chapters inbook, Monograph or books, Government report, Court or antitrust authority source, Working paper, Speech or conference presentation*)

With this notation, the model estimated by Connor and Bolotova (2006) is:

$$X_i = \alpha + \sum_i Y_i\beta_i + \sum_i Z_i\gamma + \varepsilon_i \qquad (16)$$

They estimated different restrictions of the full model. Here we consider the estimation results for the full model (column [7] of Table 6 in their paper).

Connor and Bolotova (2006) found that the estimated overcharge is positively related to the duration and does not depend on whether the firm is "guilty" or not. It is lower for domestic cartels and for cartels that have operated in the EU. Contrary to what is claimed in Cohen and Scheffman (1989), they found that the overcharge is not higher nor lower for bid-rigging cases. Also, the size of overcharges has declined over time. The authors attribute this to the increased severity of antitrust regulation. However, this effect is difficult to distinguish from the one of increased competition and free trade.

Interestingly, the Z variables also have significant impacts on overcharge estimates. The yardstick method produces estimates that are on average more than 10% higher than the "after the conspiracy" method. Otherwise, there are no significant differences between the other methods.[8] Using the "monograph or book" as reference for the type of publication, the regression of Connor and Bolotova predicted that Government reports underestimate the overcharge by 22% while court reports overestimate the overcharge by 15%. Otherwise, they observed no significant differences between the effects of other sources of publication.

The fact that the Z variables show significant effects in the regression makes the 23.3% median overcharge and the 50.4% average overcharge of Connor (2010b) difficult to interpret as indicators that meaningfully describe the behavior of *"all types of cartels over all periods of time"*. Therefore the interpretation by Connor and Bolotova (2006) of their own meta-analysis results is incomplete, partly because they fail to mention that the Z variables capture biases in the overcharge estimates, but mainly because they confuse these estimates with true unobserved overcharges. Although the regression of Connor and Bolotova (2006) may lack robustness because of the inclusion of outliers, it confirms that overcharge estimates are susceptible of being biased.

After estimating the meta-analysis model, unbiased overcharge estimates may be obtained by eliminating the estimated influence of bias factors from initial estimates. Before turning to discuss that point in detail in Section 7, we explain below the cares that must be taken when working with the Connor database.

[8] A restricted model that excludes all the Y variables predicts that the "price war" method produces estimates that are on average more than 15% higher than the "after the conspiracy" method.

5. Heterogeneity, Outliers and Sample Selection

We mentionned previously the presence of influential observations in the sample used by Connor (2010b). Unfortunately, some of these observations are included in the sample used for the meta-analysis of Connor and Bolotova (2006). The danger of including outliers in a regression analysis is expressed in the following citation from Hunter and Schmidt (2004, page 196): *"The use of least squares statistical methods [...] is based on the assumption that the data contain no aberrant values (i.e. outliers). When this assumption does not hold, the statistically optimal properties (efficiency and unbiasedness) of least squares estimates disappear. Under these circumstances, least squares estimates become very inaccurate because of their extreme sensitivity to outliers (Huber, 1980; Tukey, 1960; see also Barnett and Lewis, 1978; Grubbs, 1969)."*

In the first subsection, we illustrate the scope of the distorsion that heterogeneity in general and influential observations in particular can cause in a regression analysis. By definition, outliers represent a small number of observations and thus may be discarded from OLS regressions. But hidden forms of heterogeneity affecting a relatively large number of observations need a more careful treatment. In the second subsection, we use a K-means analysis to approximate semiparametrically the heterogeneity of cartels described by the data. The estimated heterogeneity structure is then used in subsequent analysis to improve the bias-correction of cartel overcharges. In the third subsection, we show how to correct the sample selection problem raised by the exclusion of outliers.

5.1 The Impact of Influential Observations on a Linear Regression

The presence of influential observations in a sample can be due to the fat-tailedness of the distribution that generates the data. For example, stable distributions display frequent extreme values compared to normal distributions. Influential observations can also be due to heterogeneity in the sample. For example, consider observations that are generated by two normal distributions, one with mean μ_1 and variance σ^2, and the other with mean μ_2 and variance σ^2. Let $\pi_1 = 1 - \varepsilon$ be the probability that an arbitrary observation comes from the first distribution and $\pi_2 = \varepsilon$ denote the complementary probability. For quite small ε, the number of observations coming from the second distribution can be as small as one in a large sample.

For illustration and without loss of generality, assume that we have a sample with only the first observation coming from the second distribution, and that an econometrician who ignores the heterogeneity runs the following regression:

$$X_i = \alpha + \varepsilon_i \tag{17}$$

Then the OLS estimator of α is the sample mean:

$$\widehat{\alpha} = \frac{1}{N}\sum_{i=1}^{N} X_i \quad (18)$$

To measure the impact of the unique observation generated by the second distritrution, we write:

$$\hat{\alpha} = \frac{1}{N}X_1 + \frac{N-1}{N}\left(\frac{1}{N-1}\sum_{i=2}^{N} X_i\right) \quad (19)$$

The expectation of $\hat{\alpha}$ is:

$$E(\hat{\alpha}) = \frac{1}{N}\mu_2 + \frac{N-1}{N}\mu_1 \quad (20)$$

Firstly, we see that $E(\hat{\alpha})$ lies strictly between μ_1 and μ_2 so that $\hat{\alpha}$ is biased for both μ_1 and μ_2. Second, assuming that $\mu_2 = K\mu_1$ for quite large K, we have:

$$E(\hat{\alpha}) = \frac{K+N-1}{N}\mu_1 \quad (21)$$

If the aim of the study is to draw conclusions that are applicable to the majority of the population, then the econometrician should design an estimator that targets μ_1 as precisely as possible. But the bias of $\hat{\alpha}$ for μ_1 is given by:

$$E(\hat{\alpha}) - \mu_1 = \frac{K-1}{N}\mu_1 \quad (22)$$

This bias is increasing in both K and μ_1 and can be susbtantial in many cases of practical interest. Such is the problem raised by the presence of infrequent influential observations in a sample.

With a bit more complication, similar analytical results can be showed to hold for estimated coefficients of a regression. For illustration purpose, let us consider an example where a variable *y* is linked to a variable *x* through the relation:

$$y = \alpha + \beta x + u$$

Where u is a Gaussian error with mean 0 and variance 0.25, α=1 and β=2. We simulate a sample of 50 independent observations from this model by assuming that *x* is a standard normal random variables. We then use the simulated sample to infer the intercept and slope of the model by OLS, as if they where unknown.

Next, we consider the previous sample and add +20 to the largest realization of y, leaving the corresponding *x* unchanged. One may think that the outlier hence generated is exagerated. However, in the database that is at our disposal, the largest overcharge estimate is equal to 1800, which amounts to 40 times the unconditional average overcharge estimate and 37 times the average of strictly positive overcharge estimates.

Again, we repeat the previous excercise by adding +20 to the observation of y that is immediately smaller than the previous outlier. This yields a new sample that contains two outliers. We use the three samples to estimate the intercept and slope of the linear model by OLS. The results are shown in Table 3.

Table 3.
OLS regression results with 0, 1 and 2 outliers in a sample of 50 observations.
Simulation results based on 1000 Monte Carlo replications.

	Coefficient	Mean	Std. Dev.	IC5	IC95
No outlier	α	0.999	0.071	0.884	1.118
	β	2.001	0.073	1.886	2.120
One outlier	α	1.393	0.150	1.151	1.634
	β	2.937	0.191	2.657	3.270
Two outliers	α	1.789	0.254	1.362	2.207
	β	3.713	0.287	3.285	4.216

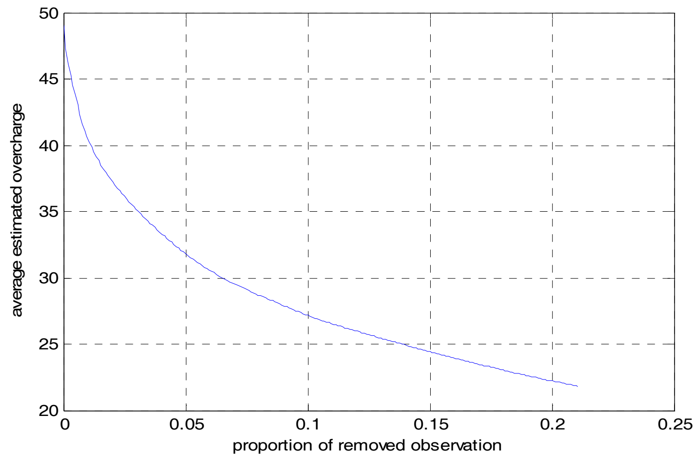
This example shows that the presence of one or two outliers in a sample of 50 observations can distort OLS results. When outliers are present, the OLS estimator are biased, their standard errors increase and the confidence intervals are misleading about the true value of the parameters. Indeed, the confidence intervals for α and β in Table 3 do not even contain the true values even with only one outlier. The relative bias of the estimator of β (the bias divided by the true value of the parameter) amounts to 47% when there is one outlier and 86% when there are two outliers. And similarly for the estimator of α. Hence if y were an overcharge and x an explanatory variable (e.g. duration of cartels), it would be wise to discard the outliers from the sample if one wish to draw conclusions that meaningfully describe the *representative* cartel.

As an empirical illustration, we compute the empirical mean of the strictly positive overcharge estimates in the Connor sample by sequentially leaving *the largest observation* out.[9] The result is shown on Figure 5. The empirical mean drops from 50% to approximately 32% when the 5% most influential observations are removed from the sample, and to approximately 22% when the 20% most influential observations are left out.

[9] Only one observation is left out at each step.

Figure 5

Impact of influential observations on the sample mean



In our empirical applications, we discard overcharge estimates that are larger than or equal to 50% from OLS regressions, which represents the 22,6% most influential observations in the sample. Zero overcharge estimates are also discarded from the meta-analysis for four reasons. First, these estimates represent 7.23% of the sample, suggesting that the probability of an overcharge *estimate* taking exactly the value zero is not equal to zero. Technically, this mean that in a regression analysis of the whole universe of overcharge estimates, the distribution of the error term should be of truncated type.[10] Second, the preferred empirical framework consists of regressing log-overcharge estimates on a number of explanatory variables. Hence zero overcharge estimates must be discarded for technical reasons (log-of-zero problem). Third, there are reasons to expect that the true underlying overcharge estimates are not all exactly equal to zero.[11] And fourth, even if the zero overcharge estimates that are present in the sample are unbiased, their empirical proportion is highly susceptible to be distorted by the publication bias.

By excluding the zeros and positive outliers, we do not mean to say that overcharges taking these values do not exist in real life. Instead, zeros and positive outliers are excluded from OLS regressIons because their inclusion would jeopardize the accuracy and generality of the results.[12]

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[10] This is not to say that overcharge data are truncated at zero. What is meant is that a truncated distribution is needed to fit the portion of the sample consisting of strictly positive overcharges, given that the true distribution admits a mass of probability at zero.

[11] In practice, zero overcharge estimates may arise either from censoring (unreported negative estimates that are set to zero) or from a mass of probability (a strictly positive proportion of unsuccessful cartels). A zero overcharge estimate belonging to the first category is likely to be biased. An example of a negative estimate found by authors and not used is discussed by Finkelstein and Levenbach (1983).

[12] Tukey (1960) and Huber (1980) recommend deletion of the largest 5% and the smallest 5% values. We may have considered winsorizing the data, which consists of replacing observations that are larger than the 80th percentile by that percentile. However, this would replace the original bias in the data by a bias of our own. Our goal is to link the true bias in the overcharge to its true potential causes (namely, the publication source and the calculation method). Moreover, the distortion introduced by the winsorization would be less

Indeed, this makes the sample selection problem more severe rather than solving it. But Heckman (1979) designed a methodology to eliminate the bias that this type of sample selection raises; we present and use this methodology in Section 6.3.

5.2. A K-means Analysis of Overcharge Data

Removing overcharge estimates that are equal to zero and those that are larger than or equal to 50% reduces the heterogeneity problem raised above, but may not provide a complete solution. We control for the residual heterogenity in the remainder of the sample by mean of a K-means analysis, which is a nonparametric cluster analysis that is aimed at partitioning a sample of observations into K groups, by minimizing the within groups heterogeneity between observations while maximizing the between groups heterogeneity. The number of groups K and the criterion used to measure the heterogeneity are chosen by the researcher. For ease of expositions, suppose we want to partition a sample $(x_1, \ldots, x_n)$ into K groups and use the Euclidian norm of the difference between two observations as the measure of heterogeneity. A typical K-means algorithm starts by a random draw of K arbitrary points $(c_1, \ldots, c_K)$ from the sample, called the centroids.

At the first step of the algorithm, each observation is allocated to the centroid to which it ressembles the most:

$$\mathrm{x_i} \in \mathrm{G_k} \Leftrightarrow \mathrm{k} = \underset{1 \leq l \leq K}{\operatorname{argmin}}\{\|\mathrm{x_i} - \mathrm{c_l}\|^2\}, \mathrm{i} = 1, \ldots, \mathrm{n} \tag{23}$$

This step partitions the sample $(x_1, \ldots, x_n)$ into K initial groups $G_1, \ldots, G_K$. At the Second step of the algorithm, one replaces the previous centroids by the average of the groups, that is:

$$\mathrm{c_k} \leftarrow \frac{1}{\mathrm{n_k}} \sum\nolimits_{\mathrm{x_i} \in \mathrm{G_k}} \mathrm{x_i}, \mathrm{k} = 1,2, \ldots, \mathrm{K} \tag{24}$$

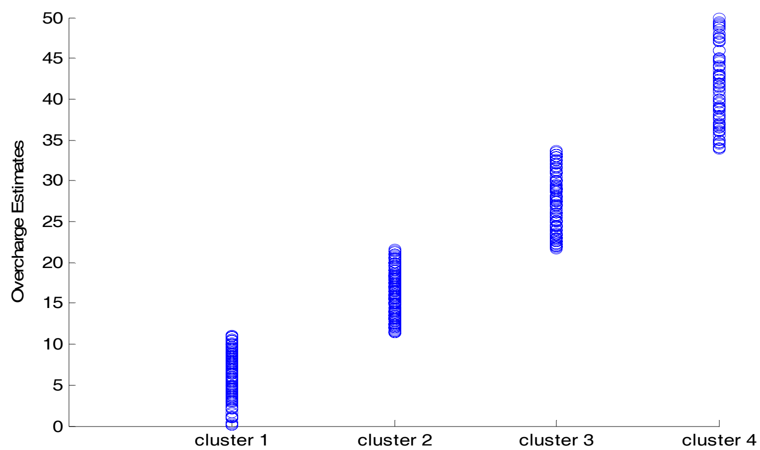
where n_k is the number of observations in the group G_k. Note that the new centroids are now fictitious observations. The third step of the algorithm consists of iterating the previous two steps until convergence, i.e. until the groups become stable. At the convergence of the algorithm, we obtain a decomposition of the original sample into K groups such that the within group variance is minimized. This is equivalent to maximizing the between group variance, as the sum of the within and between group variance gives the total variance.

We use the K-means analysis described above to segregate our sample of alleged cartels into four clusters. Variables included in the analysis are overcharge estimates and variables that may affect the size of the true overcharge (i.e. the Y variables of the meta-analysis of Connor and

pronounced if the bias where monotonically increasing in the overcharge, that is, if the bias in a 55% overcharge is higher than the bias in a 50% overcharge. Clearly, this may not be the case if the two overcharges are calculated using different methods.

Bolotova). We find that the ranges of overcharge estimates across clusters do not overlap in the multidimensional K-means analysis, which reflects the fact that heterogeneity in the sample mainly comes from the distribution of overcharge estimates.[13] The lower cluster is comprised of 215 cartels with overcharge estimates varying from 0.1% to 11.1%. The moderately low cluster gathers 234 cartels with overcharge estimates lying between 11.4% to 21.6%. The moderately high cluster contains 197 overcharge estimates lying between 21.7% and 33.7%. Finally, at the very high end, we have 140 cartels whose overcharges are estimated to lie between 33.9% to 49.9%. In the next section, this decomposition of the sample into clusters is exploited in our bias-correction scheme of overcharge estimates.

Figure 6
Separation of the overcharge estimates into homogenous clusters.
(The ranges of overcharge estimates do not overlap)



5.3. Controlling for the Sample Selection Bias

Leaving roughly 30% of cartels out of the sample[14] used for our OLS regressions may cause some of our conclusions to loose generality. This would be true even if we know that including these cartels deteriorates the results of the analysis. The absence or deletion of a non negligible proportion of observations from a study raises a sample selection problem well documented in econometrics.[15] In the present case, the deletion of observations (with original

[13] We find that the composition of clusters with multidimensinal centroids does not change very much when only overcharge estimates are used to perform the analysis. For instance, when only overcharge estimates are used in the analysis, the first cluster ranges from 0.1% to 12.5% overcharge estimates.
[14] In total, 22.59% of overcharge estimates larger than or equal to 50% and 7.23% of zero overcharge estimates.
[15] An interesting example cited in Heckman (1979) arises from the self-selection of women who choose to work outside the household. Indeed, the salary is observable only for women whose market wage exceeds the implicit home wage obtained when they do not work at all. Hence in studying the gender salary gap, it is important to bear in mind that some women choose deliberately not to work because they are not sufficiently motivated to do so (here the motivation is measured in monetary terms). The sample is necessarily truncated

overcharge estimates of 0% and of ≥50%) for estimation purposes is done to avoid obtaining distorted results due to outliers.[16]

In models that are linear in the parameters, Heckman (1979) shows that the bias induced by sample selection on regression coefficients is a missing regressor problem. To fix ideas, let us assume there exists a latent index X_i^* indicating the quality of the data.[17] In the present context, the quality of an observation is defined in relation with its contribution to the quality of the estimation results. In concrete terms, large positive overcharge estimates are considered poor quality data, not necessarily because they are measured with more bias than other observations, but because including them distorts the relevancy of the results of our analysis.[18] In the same vein, the log of zero is equal to minus infinity, hence zeros may be considered outliers in a log-linear regression analysis.

Suppose that the quality indicator X_i^* takes the following value for cartel i:

$$X_i^* = A + Y_i B + Z_i C + u_i^* \quad (25)$$

where u_i^* follows a standard normal distribution. Assume that this latent variable is such that the cartel *i* is included in the meta-analysis if and only if $X_i^* > 0$, while the cartel is exluded otherwise.[19] By definition, Equation (12) is estimated only on the portion of the sample where $X_i^* > 0$. The expectation of the overcharge estimates on this subsample is:

$$E(X_i | Y, Z, X_i^* > 0) = \alpha + Y_i\beta + Z_i\gamma + E(\varepsilon_i | X_i^* > 0) \quad (26)$$

Hence, the sample selection bias is controlled by estimating the following equation, which is Equation (12) augmented with the term $E(\varepsilon_i | X_i^* > 0)$. We have:

$$X_i = \alpha + Y_i\beta + Z_i\gamma + E(\varepsilon_i | X_i^* > 0) + e_i, \quad (27)$$

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and therefore, using it without caution to estimate the model will produce distorted results that does not reflect the situation of the typical woman.

[16] Deleting observations with overcharge estimates that are ≥50% is quite conservative in light of the economic theory on mark-ups from which it appears that overcharges of more than 10% are quite dubious. For instance, a threshold of 10% would be reasonable according to Cohen and Scheffman (1989).

[17] A latent variable is an unobserved variable that may have observable implications. A latent variable may be inferred using some of its observable implications along with mild identification assumptions.

[18] Although we suspects that these observations are more biased than others, this is not the main or only reason justifying their exclusion.

[19] This model is consistent with all situations where $X_i^* \geq b$ for zero overcharge estimates, $X_i^* < b$ for estimates lying strictly between 0% and 50% and $X_i^* \geq b$ the for observations above 50%. This amounts to assuming that the latent factor is nonlinear in the overcharge estimates. Note that there is no excluded variable in the second step. The same variables are used in both the latent variable equation and the observation equation. The nonlinearity assumption imposed on the latent variable appears to be a powerful identification tool in this model. It is also consistent with the fact that zero overcharge estimates are likely to be less biased than those larger than or equal to 50%.

where the error term $e_i \equiv \varepsilon_i - E(\varepsilon_i | X_i^* > 0)$ has zero expectation by construction. It can be shown that:

$$E(\varepsilon_i | X_i^* > 0) = \theta \frac{\varphi(A+Y_iB+Z_iC)}{\Phi(A+Y_iB+Z_iC)} \tag{28}$$

where the theoretical value of θ is $\mathrm{Corr}(\varepsilon_i, u_i)\hat{\sigma}_\varepsilon^2$ and φ and Φ are the standard normal density and cumulative distributions respectively (See Heckman, 1979).[20] Similarly, we have:

$$E(\varepsilon_i | X_i^* < 0) = -\theta \frac{\varphi(A+Y_iB+Z_iC)}{1-\Phi(A+Y_iB+Z_iC)} \tag{29}$$

Let us consider the inverse Mills ratio (IMR) defined as:

$$imr_i = \frac{\varphi(A+Y_iB+Z_iC)}{\Phi(A+Y_iB+Z_iC)} \quad \text{if } X_i^* > 0 \tag{30}$$

$$imr_i = \frac{-\varphi(A+Y_iB+Z_iC)}{1-\Phi(A+Y_iB+Z_iC)} \quad \text{if } X_i^* < 0 \tag{31}$$

When restricted to the subsample of cartels defined by $X_i^* > 0$, the estimating equation (27) is equivalent to:

$$X_i = \alpha + Y_i\beta + Z_i\gamma + imr_i\theta + e_i, \tag{32}$$

A similar methodology also applies to the log-linear model (13).[21]

If the IMR is not included in the estimating equation, the coefficients β and γ are estimated with bias. In this case, the model can still be used to predict the mean overcharge conditional on $X_i^* > 0$, but not conditional on Y or Z. Given this, only the model that controls for sample selection can be used to bias-correct individual overcharge estimates.

A probit can be estimated to infer fitted values of X_i^* and the IMR. Such a probit analysis is interesting per se because it permits to see the categories of cartels that have been excluded the most. We do not observe the latent variable, but we do know if an observation is excluded (I_i=0) or not (I_i=1). If observation i is not excluded, then it must be the case that $X_i^* > 0$, which in turn implies that $A + Y_iB + Z_iC + u_i^* > 0$, or equivalently, $-u_i^* < A + Y_iB + Z_iC$. Because u_i^* is standard normal, the likelihood of this observation is equal to:

$$L_i = \Phi(A + Y_iB + Z_iC) \tag{33}$$

[20] This result is obtained by exploiting the definition of $E(\varepsilon_i | X_i^* > 0)$, which is the expectation of ε with respect to its density conditional on $-u_i^* < A + Y_iB + Z_iC$. This density is of truncated type and is obtained by exploiting the bivariate Gaussian distribution of $(\varepsilon i, u_i^*)$. See Heckman (1979) for details.

[21] The same inverse Mills ratio is used in both the linear and log-linear model. At this point, it is difficult to tell which model will provide the best fit.

where Φ is the cumulative distribution function of the standard normal. Likewise, if observation i is excluded, then it must be the case that $X_i{}^* < 0$, which in turn implies that $-u_i^* > A + Y_i B + Z_i C$. Hence the likelihood of an excluded observation is given by $1 - L_i$, where L_i is defined above. The sample log-likelihood of the probit is thus given by:

$$\mathcal{L}(A, B, C) = \sum_{i=1}^{T} \{I_i * \log L_i + (1 - I_i) * \log (1 - L_i)\} \tag{34}$$

Maximizing this log-likelihood with respect to parameters A, B and C gives the probit estimators $\widehat{A}$, $\widehat{B}$ and $\widehat{C}$, whose values are reported in Table 4.

The probit results show that international cartels have been excluded more than domestic cartels as the latter coefficient is positive and significant.[22] This is consistent with the fact that in the Connor sample, the overcharge estimates of international cartels are higher than those of domestic cartels. We also see that cartels involved in bid-rigging have been excluded less than those involved in other price fixing collusions. Cartels that pleaded guilty have been excluded less than those that did not. Cartels of the ROW have been excluded less than those of the benchmark category WORLD, but the difference if not statistically significant. In trun, the WORLD cartels have been excluded less than those of the US, EU and ASIA groups. Finally, cartels of the period P6 have been excluded less than those of periods P1 to P5.

Estimates obtained by the econometric method (used as benchmark) have been excluded less than those computed by other methods as the coefficients of all the methods are negative and significant. This indicates that the use of econometric methods in the original studies tend to conclude with overcharge estimates in the interval 0% to 50%. Finally, estimates published in government report (used as benchmark) have been discarded more than those published in other publication sources as the coefficients of all the publication media are positive.

[22] We consider a one-sided test and 10% significance level.

Table 4
A probit model for the inclusion of cartels into the meta-analysis
(Variables with positive coefficients are correlated with inclusion, and vice versa)

	Coefficients	Student-t[23]
Constant	0.03	0.10
Duration	-0.01	-0.22
Domestic	0.61	5.29
BidRig	0.49	3.65
Guilty	0.15	1.29
US	-0.41	-2.83
EU	-0.15	-1.19
ASIA	-0.56	-2.86
ROW	0.29	1.02
P1	0.44	0.83
P3	0.14	0.69
P4	0.22	1.04
P5	0.02	0.10
P6	0.28	1.37
OTHER	-0.77	-3.75
HISTOR	-1.40	-4.01
PBEFOR	-0.24	-1.55
PWAR	-0.63	-2.02
PAFTER	-0.29	-1.66
COST	-0.48	-2.14
YARDST	-0.44	-2.54
LEGAL	0.09	0.46
JOURNAL	0.25	1.31
MONOGR	0.37	2.08
EDBOOK	0.15	0.61
COURT	0.35	1.73
WORKP	0.81	3.76
SPEECH	1.16	1.71
Sample size[24]	1119	

Let $\widehat{X}_i^* = \widehat{A} + Y_i\widehat{B} + Z_i\widehat{C}$ be the fitted values of X_i^*. Empirically, the sample selection bias is controlled in the meta analysis by including the estimated inverse Mills ratio as an additional regressor. For an included cartel, we have:

$$\widehat{\imath mr}_i = \frac{\varphi(\widehat{X}_i^*)}{\Phi(\widehat{X}_i^*)} \tag{35}$$

[23] A coefficient is significant in a one sided 10% level test if the corresponding student-t is larger than 1.28 in absolute value.
[24] In the original database, there is one overcharge estimate with publication source labelled as "OTHER". This observation has been excluded from the meta-analysis.

Heckman (1979) shows that including $\widehat{\mathrm{imr}}_i$ as an additional regressor in (12) and (13) allows to consistently estimate the coefficients of the model linking the dependent variable (original overcharge estimates) to the explanatory variables Y and Z, almost as if the whole sample were available or used. The estimating equation is thus given by:

$$X_i = \alpha + Y_i\beta + Z_i\gamma + \widehat{\mathrm{imr}}_i\theta + e_i. \tag{36}$$

The coefficients estimated from (36) using the subsample of positive but less than 50% overcharge estimates can be used to bias-correct the overcharge estimates that have been included in the regression and those that have been excluded as well.[25] The bias-correction formulas are presented in detail in Section 6 below.

In summary, the approach taken in our meta-analysis is as follows. First, outliers are excluded to avoid distorting the relevancy of the regression results. Second, we construct nonparametrically four clusters (groups of cartels), whose indicators are included in the regression analysis in interaction with the bias factors (the Z variables) to control for the remaining heterogeneity in the data. Third, a probit analysis is conducted from which an inverse Mills ratio is generated and included in the regression analysis to correct for the non-random nature of the truncation of the sample. Fourth, regression models linking the original overchage estimates to explanatory variables Y and Z as well as to the predicted value of the inverse Mills ratios obtained from the probit analysis are estimated. Finally, bias-corrected mean and median estimates are obtained for different subgroups of cartels.

6. Bias-correcting the Initial Overcharge Estimates

We estimate both the linear and log-linear model using the same Y and Z variables as in Connor and Bolotova (2006).[26] We estimate the models with and without controlling for sample selection bias. The estimated coefficients for the Y variables are shown in Table 5 while those of the Z variables are shown in Appendix B. The coefficients of the Z variables in clusters 1 and 2 are generally negative and significant while those of clusters 3 and 4 are generally positive and significant. This confirms that the sample of overcharge estimates is heterogenous. These discrepancies play an important role in explaining the large magnitude of the bias.

[25] The formula of the IMR for an excluded cartel only serves for bias-correction. See section 6.

[26] Even with no interaction, the coefficients of the Z variables are not directly comparable with the results of Connor and Bolotova (2006) because we have changed the reference variables. Connor and Bolotova (2006) used PAFTER and MONOGR as references variables for the method of calculation and source of publication, while we use ECON and GOVREP respectively. Also, the modality P6 of the antitrust law regime now covers periods from 1991 to 2010.

We see two main differences with respect to the results of Connor and Bolotova (2006). First, the coefficient of the duration variable is not statistically significant in a conventional 5% level two-sided test. In the log-linear model, this coefficient is significant at level 10% in a one sided test, but is also estimated to be low (0,03%). This finding suggests that the ability of a cartel of five to ten year duration to raise its price is not very different from that of a cartel of zero to five year duration. Second, the results from the log-linear model suggest that cartels resolved with plea agreements display significantly higher overcharges than other cartels. Otherwise, our results are qualitatively similar to those of Connor and Bolotova (2006) regarding the signs of the estimated coefficients. To choose between the linear and log-linear model, we compare the variance ratio (variance explained ove total variance). This ratio is 11.16% for the linear model and 13.40% for the log-linear model. Hence, the log-linear model explains the variance of the original estimates to a greater extent and for this reason is the preferred model.

After estimating the meta-regression model (with or without controlling for selection bias), bias-corrected overcharge estimates can be computed. Bias-corrected estimates are obtained by eliminating the impact of the Z variables from a given initial estimator X_i.

In the linear model, the expected bias-corrected overcharge estimates are given by:

$$\widehat{\mathrm{X}}_{\mathrm{i}} = \widehat{\alpha} + \mathrm{Y}_{\mathrm{i}}\widehat{\beta} + \widehat{\mathrm{\imath mr}}_{\mathrm{i}}\widehat{\theta} \tag{37}$$

where $\widehat{\mathrm{\imath mr}}_{\mathrm{i}}$ is given by Equation (30) for included cartels and by Equation (31) for excluded cartels.[27] In the log-linear model, the bias-corrected overcharge estimate is given by:[28]

$$\widehat{\mathrm{X}}_{\mathrm{i}} = \exp\left(\widehat{\alpha} + \mathrm{Y}_{\mathrm{i}}\widehat{\beta} + \widehat{\mathrm{\imath mr}}_{\mathrm{i}}\widehat{\theta} + \widehat{\sigma}_{\mathrm{e}}^{2}/2\right), \tag{38}$$

assuming that $e_i \sim N(0, \sigma_{\mathrm{e}}^2)$. The contribution of the IMR reflects the amount by which the expected overcharge of a cartel deviates from the expected overcharge over the whole sample, given that this cartel is included or not. The presence of the IMR in the bias-correction formula allows to fully exploit the whole information set that conditions the estimation, which is the knowledge of the regressors and the knwoledge of the selection criterion. Again, the formula of the IMR depends on whether the cartel is included in the regression or not.

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[27] In a previous version of this paper, we did not use the IMR in the bias-correction formula of excluded cartels. It appears that this is an important omission since excluded cartels are also subject to an "exclusion" bias that is proportional to (31).

[28] In the lognormal framework, the average overcharge of an included cartel is equal to $\exp\left(\widehat{\alpha} + \mathrm{Y}_{\mathrm{i}}\widehat{\beta} + \widehat{\mathrm{\imath mr}}_{\mathrm{i}}\widehat{\theta}\right)$ multiplied by the smearing factor $\exp(\widehat{\sigma}_{\mathrm{e}}^{2}/2)$.

Table 5

Estimation Coefficients for the Y variables

Mean overcharge 1 is the estimated average bias-corrected (bc) overcharge for the subsample of positive initial estimates below 50%. *Mean overcharge 2* is the same for the whole sample.

		Linear regression		Log-linear regression	
		Coefficients	Student-t	Coefficients	Student-t
No selection bias control	Constant	17.26	13,80	2,35	16,32
	Duration	0.06	0,34	0,03	1,49
	Domestic	-0.51	-1,17	-0,03	-0,67
	BidRig	-0.37	-0,86	-0,03	-0,58
	Guilty	0.68	1,53	0,20	3,88
	US	0.38	0,65	0,02	0,32
	EU	0.54	1,06	0,01	0,21
	ASIA	1.17	1,59	0,05	0,64
	ROW	0.34	0,38	0,10	1,00
	P1	-1.31	-0,66	-0,01	-0,03
	P3	-1.19	-1,30	-0,15	-1,45
	P4	-1.39	-1,54	-0,18	-1,75
	P5	-0.92	-1,00	-0,11	-1,00
	P6	-0.56	-0,65	-0,14	-1,45
	Var(epsilon)	18.15		0,24	
	Mean bc overcharge 1	17.18		12.09	
	Variance ratio	11.16		13,40	
	Sample size	786			
Controlling Selection bias	Constant	16.72	4,60	2,85	6,80
	Duration	0.05	0,33	0,03	1,56
	Domestic	-0.32	-0,25	-0,21	-1,41
	BidRig	-0.25	-0,28	-0,14	-1,38
	Guilty	0.74	1,24	0,14	2,04
	US	0.25	0,25	0,14	1,21
	EU	0.49	0,81	0,06	0,84
	ASIA	1.00	0,78	0,21	1,40
	ROW	0.40	0,41	0,05	0,45
	P1	-1.16	-0,53	-0,14	-0,57
	P3	-1.16	-1,22	-0,19	-1,71
	P4	-1.33	-1,35	-0,24	-2,11
	P5	-0.90	-0,96	-0,13	-1,20
	P6	-0.46	-0,44	-0,23	-1,91
	IMR	0.66	0,16	-0,61	-1,25
	Var(epsilon)	18.18		0,24	
	Mean bc overcharge 1	17.11		13.62	
	Mean bc overcharge 2	15.79		17.52	
	Variance ratio	11. 15		15.56	
	Sample size	786			

In the log-linear model, the estimated variance for the error term is 0.24 in both cases, that is, when sample selection is controlled and when it is not. However, the percentage of variance explained by the bias corrected overcharge estimate is higher when sample selection is controlled compared to when it is not. This difference comes from the inclusion of the IMR which alters the estimated values for all coefficients. In particular, it alters the estimated proportion of variance due to the bias factors (the Z variables).

We have applied the bias-correction formulas given above only to cartels with initially positive overcharge estimate. Two reasons explain this choice. First, the empirical distribution of overcharge estimates exhibits a mass at zero. This means that neither the linear model nor the log-linear model can fit this distribution at zero.[29] Second, the predictions of the log-linear model are necessarily positive, which indicates that this model always overestimates zero overcharges. Thus, and for simplicity, we report a 0% bias-corrected overcharge for a cartel with an initial estimate equal to zero.[30]

In Table 6, we present the average bias-corrected overcharge estimates for the categories of cartels prevously considered in Table 1. The log-linear model predicts for the subsample with initial estimates lying in the range 0% to 50% a mean overcharge estimate of 13.62% with a median of 13.63% and for all cartels of all types a mean of 17.52% with a median of 14.05%. Both mean and median overcharge estimates are of interest here. Indeed, the mean is roughly identical to the median for the subsample while the mean is significantly larger than the median for the whole sample. This indicates that the bias-corrected overcharges estimated for the subsample with initial estimates above 50% contains outliers. Hence, the median should be given more consideration than the mean when the whole sample is considered. The median is 14.05% for the whole sample, which is quite close to the mean of 13.62% obtained for the subsample with initial estimates lying in the range 0% to 50%.

In a standard Heckman analysis, the portion of the sample that is excluded is not observed and hence, the analysis would stop with the first set of estimated results (mean overcharge estimate of 13.62% with a median of 13.63%). In our case, the regressors are observed for the all obervations. The excluded observations have been excluded because of the realizations of the dependent variables (the initial overcharge estimates) for these observations which are too influential to be included in the same regression analysis as the remainder of the sample. Moreover, these high initial overcharge estimates are not supported by any economic theory. If the quality of the observations were the same for the whole sample, an approach inspired from the treatment

[29] In fact, these models are not rich enough for the purpose of bias-correcting zero overcharges.
[30] This may affect the average bias-corrected overcharge over the whole sample (the "All cartels" column of Table 6 below), but it does not affect the average of positive estimates.

effect literature could have been used. In the latter approach, all observations are included in the regession of the X or logX onto Y, Z and the IMR, with the formula of the IMR depending on whether the realization of X lies above 50% or not. To accomodate the nature of the data, the approach that we advocated is halfway. It consists first of estimating the parameters of interest using a Heckman regression as if the bad quality observations were not available. Next, the estimated coefficients are used together with the realizations of the regressors to predict the dependent variables for the bad quality observations. These predictions give an assessment of what the realizations X or logX should have been if they were measured without bias.

The log-linear model predicts for US cartels a mean overcharge estimate of 13.73% (with a median of 13.75%) for the subsample and 16.45% (with a median of 14.01%) for the whole sample. For EU cartels, the corresponding figures are 13.08% (13.32%) and 17.75% (13.78%). The model predicts moreover that the mean overcharge estimate of international cartels are larger than that of domestic cartels by 0.73 (or 5.5% more profitable) and 4.20 percentage points (or 27.5% more profitable) for the subsample and the whole sample respectively (not considering the potentially different costs of forming and maintaining international versus domestic cartels). Finally, it predicts that recent cartels (post-1973) achieved slightly lower mean but slightly larger median overcharges than cartels of the more distant past (pre-1973).

An important feature of Table 6 is that the mean and median bias-corrected overcharge estimates shows a more homogenous behaviour of cartels accross different types, geographical locations and periods than suggested by the raw data of Table 1. In a sense, this suggests that *a cartel is a cartel is a cartel.*

Table 7 shows average bias-corrected overcharge estimates for different categories of cartels according to whether they are domestic or international, in bid-rigging cases of not, and/or were found or pleaded guilty or not. Table 8 shows median bias-corrected overcharge estimates for the same subgroups. The differences between the raw overcharge estimates (Table 1 and the left-hand side of Tables 7 and 8) and the bias-corrected ones (Table 6 and the right-hand side of Tables 7 and 8) are striking, both in terms of levels and in terms of orderings. The medians of the subgroups do not change too much as one moves from the whole sample to the subsample with initial estimates lying within the range 0%-50%. This suggests that the subsample is quite representative of the whole universe of cartels as long as the median bias-corrected overcharge estimate is given more consideration than the mean. Finally, note that the mean and median overcharge estimates in Tables 7 and 8 are sometimes higher in the subsample than in the whole sample. This stems from the fact that zero estimates are part of the whole sample while they are excluded from the subsample.

Table 6

Bias-corrected mean and median overcharge estimates (OE)
The prop.% are fractions of the total Connor sample (1120 cartels)

		All Cartels	OE > 0%	0% < OE < 50%	OE ≥ 50%	Cartels Before 1973	Cartels After 1973
All locations	Mean	17,52	18,89	13,62	35,28	17,87	17,39
	Median	14,05	14,35	13,63	34,23	13,45	14,22
	prop.	100,00	92,77	70,18	22,59	28,50	71,50
US	Mean	16,45	18,13	13,73	34,70	17,92	15,76
	Median	14,01	14,27	13,75	32,52	13,94	14,14
	prop.	30,00	27,23	21,52	5,71	9,64	20,36
EU	Mean	17,75	18,97	13,08	34,16	17,70	17,79
	Median	13,78	13,90	13,32	32,55	13,40	14,03
	prop.	33,48	31,34	22,59	8,75	13,39	20,09
Domestic	Mean	15,29	16,80	13,26	37,00	16,86	14,59
	Median	13,40	13,55	13,32	35,80	13,40	13,39
	prop.	46,79	42,59	36,25	6,34	14,46	32,32
International	Mean	19,49	20,67	13,99	34,61	18,90	19,70
	Median	15,44	15,64	14,03	33,91	13,67	15,52
	prop.	53,21	50,18	33,93	16,25	14,02	39,20

Table 7
Raw versus Bias-corrected mean cartel overcharge estimates.
The values reported in the last group of columns are the empirical averages of the bias-corrected overcharges predicted by the log-linear model, ***controlling for sample selection***.
"n.a." means that the corresponding category of cartels is not represented in the sample or subsample.

Cartel characteristics			Raw Average Estimates					Bias-Corrected Average Estimates				
domestic	bidrig	guilty	US	EU	ASIA	ROW	WORLD	US	EU	ASIA	ROW	WORLD
Subsample with initial overcharge estimates lying between 0% and 50%												
Yes	Yes	Yes	19,69	15,56	25,65	17,40	18,70	13,61	13,06	13,47	12,77	12,62
No	Yes	No	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
Yes	No	No	18,88	17,60	20,49	18,67	n.a.	12,05	11,8	11,46	12,21	n.a.
No	No	Yes	19,27	21,25	17,25	21,21	23,85	15,50	14,42	15,43	15,39	15,03
Yes	Yes	No	11,57	16,67	4,88	14,50	n.a.	11,44	10,68	12,32	11,73	n.a.
No	Yes	Yes	22,20	18,20	25,53	28,73	10,23	14,60	14,07	14,78	16,50	14,41
Yes	No	Yes	18,70	17,77	22,39	11,41	n.a.	15,05	13,95	14,11	13,82	n.a.
No	No	No	29,50	23,29	29,00	n.a.	23,97	11,12	10,94	10,92	n.a.	11,35
Whole sample												
Yes	Yes	Yes	26,73	17,38	29,47	17,40	18,70	15,93	13,06	17,61	12,77	12,62
No	Yes	No	n.a.	430,00	n.a.	n.a.	n.a.	n.a.	28,55	n.a.	n.a.	n.a.
Yes	No	No	46,72	20,15	77,15	17,12	n.a.	16,31	12,21	15,23	11,19	n.a.
No	No	Yes	27,67	52,78	37,60	24,49	71,79	19,28	21,41	21,35	18,32	21,11
Yes	Yes	No	21,18	16,67	4,88	14,50	n.a.	15,68	10,68	12,32	11,73	n.a.
No	Yes	Yes	38,05	32,89	35,40	28,73	20,18	20,44	21,19	19,97	16,50	19,26
Yes	No	Yes	44,51	20,52	51,24	11,41	n.a.	15,63	16,39	18,87	13,82	n.a.
No	No	No	46,56	72,20	29,00	50,00	34,29	16,47	19,80	10,92	23,24	12,56

Table 8

Raw versus Bias-corrected median cartel overcharge estimates.

The values reported in the last group of columns are the empirical medians of the bias-corrected overcharges predicted by the log-linear model, ***controlling for sample selection***.

"n.a." means that the corresponding category of cartels is not represented in the sample or subsample.

Cartel characteristics			Raw Median Estimates					Bias-Corrected Median Estimates				
domestic	bidrig	guilty	US	EU	ASIA	ROW	WORLD	US	EU	ASIA	ROW	WORLD
Subsample with initial overcharge estimates lying between 0% and 50%												
Yes	Yes	Yes	17,40	12,30	28,80	17,40	18,70	13,75	12,59	13,47	12,77	12,62
No	Yes	No	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
Yes	No	No	18,25	15,65	19,00	17,50	n.a.	12,03	11,85	11,17	11,95	n.a.
No	No	Yes	10,40	19,23	17,50	20,30	22,30	15,57	14,12	15,64	15,62	15,44
Yes	Yes	No	8,00	17,00	4,83	14,50	n.a.	11,34	10,65	12,90	11,73	n.a.
No	Yes	Yes	20,50	14,00	18,10	30,55	11,90	14,45	14,01	13,93	16,58	13,67
Yes	No	Yes	15,50	15,10	20,00	10,00	n.a.	15,19	13,55	13,90	13,46	n.a.
No	No	No	30,50	23,50	29,00	n.a.	26,25	11,39	10,98	10,92	n.a.	11,66
Whole sample												
Yes	Yes	Yes	18,05	12,15	29,00	17,40	18,70	13,76	12,59	13,47	12,77	12,62
No	Yes	No	n.a.	430,00	n.a.	n.a.	n.a.	n.a.	28,55	n.a.	n.a.	n.a.
Yes	No	No	24,70	13,50	24,50	13,60	n.a.	12,30	11,48	11,47	11,95	n.a.
No	No	Yes	27,80	29,00	17,50	21,65	29,60	16,25	15,40	15,64	15,68	15,86
Yes	Yes	No	14,50	17,00	4,83	14,50	n.a.	11,50	10,65	12,90	11,73	n.a.
No	Yes	Yes	25,75	17,00	29,80	30,55	13,45	15,18	14,65	15,48	16,58	15,00
Yes	No	Yes	14,90	15,95	23,25	10,00	n.a.	15,01	13,76	14,46	13,46	n.a.
No	No	No	36,50	50,00	29,00	50,00	25,00	11,59	25,39	10,92	23,24	11,66

7. Conclusion

The study determines the typical mean and median cartel overcharges by performing a meta-analysis similar to the one of Connor and Bolotova (2006). Our study is based on an extended version of the database used in Connor (2010b). First, we recall that the sample consists of estimates of overcharges rather than natural observations. Thus, each observation of the sample is subject to potential estimation bias, model errors and publication bias. Second, a quick examination of the sample reveals an asymmetric empirical distribution, the presence of outliers and a significant amount of heterogeneity. Although the outliers are few in number, they are able to push the mean of strictly positive overcharge estimates up to 49%. Indeed, this mean drops to 32% when the 5% most influential observations are removed from the sample. This empirical mean further drops to 20.6% when overcharge estimates that are larger than or equal to 50% are removed. Moreover, even if the data were exempt of estimation bias, model errors and publication bias, performing an OLS regression without properly addressing the problem of asymmetry, heterogeneity and outliers will necessarily produce distorted results.

We address the asymmetry problem by taking the logarithm of overcharge estimates. In order to improve the relevancy and quality of the results of the meta-analysis, overcharge estimates that are larger than or equal to 50% (22.6% of the sample) are first excluded from the estimation. Zero overcharge estimates (7.2% of the sample) are also excluded because the log of zero is either equal to minus infinity (an outlier) or undefined. To control the sample selection bias raised by the exclusion of a portion of the sample, a probit regression is estimated from which an inverse Mills ratio is obtained and included as a regressor in subsequent regressions (Heckman 1979).

Focusing on the subsample of included cartels (786 cartels), we regress the log of overcharge estimates on a number of explanatory variables. A first group of regressors Y captures the true overcharge, while a second group Z captures potential biases. To control heterogenity, a cluster analysis is used to segregate the included cartels into four homogenous clusters. The cluster indicators are then interacted with the variables capturing the biases, yielding a log-linear model in which the coefficients of the bias-correction exercise differ across clusters. The log-linear model produces a better fit compared to the linear model.

Our results show that the size of the bias depend on the overcharge estimation method and the publication media of the original estimates. The meta-analysis predicts that, controlling for sample selection, the mean bias-corrected overcharge estimate is about 13.6% (with a median of 13.6%) for the subsample of cartels with initial estimates lying between 0% and 50% and 17.5% (with a median of 14.05%) for the whole sample. These results differ significantly from the raw figures (Table 1) which are 20.61% (with a median of 18.38%) and 45.47% (with a median of 23.00%) for the subsample and the whole sample respectively.

The comparison of mean and median bias-corrected overcharge estimates accross different types, geographical locations and periods reveals a fairly homogenous behavior of cartels, a rather interesting feature that differs significantly from the highly heterogeneous behavior observed in the raw data.

Appendix A: Summary of the Variables

A1. Whole Sample

	mean	std	min	max
Overcharge Estimates	45.47	102.86	0,00	1800,00
Duration	9.25	11.86	1	109
duration (discrete)	1.86	1.06	1	4
Domestic	0.47	0.50	0	1
BidRig	0.20	0.40	0	1
Guilty	0.66	0.48	0	1
US	0.30	0.46	0	1
EU	0.33	0.47	0	1
ASIA	0.09	0.28	0	1
ROW	0.04	0.19	0	1
WORLD	0.24	0.43	0	1
P1	0.01	0.08	0	1
P2	0.10	0.30	0	1
P3	0.11	0.31	0	1
P4	0.15	0.35	0	1
P5	0.57	0.50	0	1
P6	0.07	0.25	0	1
OTHER	0.06	0.24	0	1
HISTOR	0.02	0.13	0	1
PBEFOR	0.27	0.44	0	1
PWAR	0.02	0.14	0	1
PAFTER	0.13	0.34	0	1
COST	0.05	0.21	0	1
YARDST	0.14	0.35	0	1
ECON	0.14	0.34	0	1
LEGAL	0.18	0.38	0	1
JOURNAL	0.22	0.41	0	1
MONOGR	0.23	0.42	0	1
EDBOOK	0.06	0.24	0	1
GOVREP	0.23	0.42	0	1
COURT	0.18	0.38	0	1
WORKP	0.01	0.07	0	1
SPEECH	0.07	0.25	0	1
Sample size		1120		

A2. Included cartels

	mean	std	min	max
Overcharge Estimates	20.61	12.34	0.10	49.90
Duration	9.26	11.95	1	98
duration (discrete)	1.84	1.05	1	4
Domestic	0.52	0.50	0	1
BidRig	0.24	0.43	0	1
Guilty	0.72	0.45	0	1
US	0.31	0.46	0	1
EU	0.32	0.47	0	1
ASIA	0.09	0.29	0	1
ROW	0.04	0.20	0	1
WORLD	0.24	0.43	0	1
P1	0.01	0.09	0	1
P2	0.09	0.28	0	1
P3	0.10	0.30	0	1
P4	0.13	0.34	0	1
P5	0.62	0.49	0	1
P6	0.05	0.23	0	1
OTHER	0.05	0.21	0	1
HISTOR	0.01	0.08	0	1
PBEFOR	0.27	0.44	0	1
PWAR	0.02	0.13	0	1
PAFTER	0.13	0.34	0	1
COST	0.04	0.20	0	1
YARDST	0.13	0.34	0	1
ECON	0.16	0.37	0	1
LEGAL	0.19	0.39	0	1
JOURNAL	0.22	0.41	0	1
MONOGR	0.21	0.41	0	1
EDBOOK	0.05	0.21	0	1
GOVREP	0.25	0.43	0	1
COURT	0.22	0.41	0	1
WORKP	0.01	0.08	0	1
SPEECH	0.05	0.22	0	1
Sample size		786		

A3. Cartels with Overcharge Estimate Equal to zero (Excluded)

	mean	std	min	max
Overcharge Estimates	0	0	0	0
duration	9.38	12.27	1	72
duration (discrete)	1.86	1.10	1	4
domestic	0.58	0.50	0	1
BidRig	0.07	0.26	0	1
Guilty	0.37	0.49	0	1
US	0.38	0.49	0	1
EU	0.30	0.46	0	1
ASIA	0.09	0.28	0	1
ROW	0.04	0.19	0	1
WORLD	0.20	0.40	0	1
P1	0.00	0.00	0	0
P2	0.19	0.39	0	1
P3	0.05	0.22	0	1
P4	0.25	0.43	0	1
P5	0.47	0.50	0	1
P6	0.05	0.22	0	1
OTHER	0.16	0.37	0	1
HISTOR	0.14	0.34	0	1
PBEFOR	0.20	0.40	0	1
PWAR	0.01	0.11	0	1
PAFTER	0.11	0.32	0	1
COST	0.04	0.19	0	1
YARDST	0.09	0.28	0	1
ECON	0.02	0.16	0	1
LEGAL	0.23	0.43	0	1
JOURNAL	0.44	0.50	0	1
MONOGR	0.21	0.41	0	1
EDBOOK	0.10	0.30	0	1
GOVREP	0.09	0.28	0	1
COURT	0.05	0.22	0	1
WORKP	0.00	0.00	0	0
SPEECH	0.11	0.32	0	1
Sample size		81		

A4. Cartels with Overcharge Estimate Larger than or Equal to 50% (Excluded)

	mean	std	min	max
Overcharge Estimates	137.26	188.29	50.00	1800.00
duration	9.18	11.50	1	109
duration (discrete)	1.93	1.08	1	4
domestic	0.28	0.45	0	1
BidRig	0.10	0.30	0	1
Guilty	0.57	0.50	0	1
US	0.25	0.44	0	1
EU	0.39	0.49	0	1
ASIA	0.08	0.28	0	1
ROW	0.02	0.12	0	1
WORLD	0.26	0.44	0	1
P1	0.01	0.09	0	1
P2	0.13	0.34	0	1
P3	0.15	0.35	0	1
P4	0.16	0.37	0	1
P5	0.45	0.50	0	1
P6	0.11	0.31	0	1
OTHER	0.08	0.28	0	1
HISTOR	0.01	0.09	0	1
PBEFOR	0.28	0.45	0	1
PWAR	0.03	0.18	0	1
PAFTER	0.14	0.35	0	1
COST	0.06	0.24	0	1
YARDST	0.20	0.40	0	1
ECON	0.08	0.28	0	1
LEGAL	0.11	0.31	0	1
JOURNAL	0.16	0.37	0	1
MONOGR	0.30	0.46	0	1
EDBOOK	0.10	0.30	0	1
GOVREP	0.22	0.42	0	1
COURT	0.10	0.30	0	1
WORKP	0.00	0.06	0	1
SPEECH	0.11	0.31	0	1
Sample size		253		

Appendix B: Estimated coefficients for the bias factors.

In the tables, the Xk denote the interaction variable between X and cluster indicator k. HISTOR2 and HISTOR3 are missing because they are empty (identically zero). We see that the coefficients vary a lot across clusters. For example, the coefficient of JOURNAL1 is -9.51 (in cluster 1) while that of JOURNAL2 is -0.65 (in cluster 2), that of JOURNAL3 is 9.88 (in cluster 3) and that of JOURNAL4 is 15.54 (in cluster 4). [31] These large variations in the coefficients of the Z variables reflect the large magnitude of the bias of the initial overcharge estimates.

B1. No sample Selection bias control

	Linear regression		Log-linear regression	
	Coefficients	Student-t	Coefficients	Student-t
OTHER1	-6.19	-4.58	-0.77	-4.94
HISTOR1	-4.26	-1.62	-0.70	-2.32
PBEFOR1	-2.87	-3.03	-0.15	-1.33
PWAR1	2.50	0.57	0.65	1.28
PAFTER1	-3.18	-2.65	-0.55	-3.99
COST1	-5.33	-2.72	-0.97	-4.28
YARDST1	-3.23	-3.04	-0.21	-1.69
LEGAL1	-3.12	-3.15	-0.23	-1.98
JOURNAL1	-9.51	-9.30	-0.74	-6.31
MONOGR1	-6.36	-5.17	-0.21	-1.46
EDBOOK1	-9.50	-5.08	-0.57	-2.65
COURT1	-7.37	-6.52	-0.43	-3.32
WORKP1	-6.82	-5.82	-0.34	-2.52
OTHER2	-0.12	-0.06	0.05	0.19
PBEFOR2	-0.16	-0.17	0.12	1.15
PWAR2	-0.27	-0.14	0.10	0.42
PAFTER2	0.26	0.26	0.10	0.86
COST2	-0.22	-0.17	0.08	0.53
YARDST2	0.34	0.34	0.11	0.91
LEGAL2	0.54	0.52	0.13	1.12
JOURNAL2	-0.65	-0.58	0.30	2.36
MONOGR2	-0.82	-0.71	0.30	2.26
EDBOOK2	0.58	0.39	0.50	2.90
COURT2	-1.74	-1.48	0.19	1.41
WORKP2	-1.77	-1.61	0.24	1.91
SPEECH2	-4.99	-1.04	0.06	0.10

[31] A parameter is significant at level 10% (resp: 5.%) in a two-sided test if its Student-t is larger than 1.64 (resp: 1.96) in absolute value. In a one sided test, the thresolds is $\pm$ 1.28 at level 10% and $\pm$ 1.64 at level 5%.

B1. No sample Selection bias control (Continued)

	Linear regression		Log-linear regression	
	Coefficients	Student-t	Coefficients	Student-t
OTHER3	4.71	2.45	0.37	1.68
PBEFOR3	1.20	1.33	0.18	1.70
PWAR3	1.60	0.61	0.21	0.70
PAFTER3	0.80	0.65	0.18	1.30
COST3	2.70	1.62	0.29	1.50
YARDST3	1.22	1.04	0.14	1.04
LEGAL3	0.10	0.08	0.12	0.80
JOURNAL3	9.88	8.63	0.81	6.16
MONOGR3	9.18	7.90	0.76	5.64
EDBOOK3	9.05	5.45	0.84	4.41
COURT3	9.09	6.86	0.70	4.57
WORKP3	8.73	7.75	0.72	5.57
SPEECH3	5.68	1.74	0.64	1.71
OTHER4	12.74	7.93	0.69	3.73
HISTOR4	10.01	3.09	0.55	1.48
PBEFOR4	10.13	8.56	0.57	4.21
PWAR4	6.83	2.71	0.44	1.51
PAFTER4	9.81	7.02	0.57	3.55
COST4	11.38	4.20	0.56	1.79
YARDST4	10.01	7.29	0.57	3.60
LEGAL4	8.62	5.70	0.51	2.92
JOURNAL4	15.54	12.79	0.85	6.06
MONOGR4	13.29	9.79	0.75	4.82
EDBOOK4	21.13	13.22	1.19	6.45
COURT4	13.06	9.44	0.68	4.23
WORKP4	14.60	10.54	0.73	4.59
SPEECH4	10.42	3.01	0.69	1.73

B2. Controlling for sample selection bias

	Linear regression		Log-linear regression	
	Coefficients	Student-t	Coefficients	Student-t
OTHER1	-6.43	-3.13	-0.55	-2.31
HISTOR1	-4.77	-1.14	-0.24	-0.49
PBEFOR1	-2.93	-2.89	-0.09	-0.80
PWAR1	2.31	0.50	0.83	1.57
PAFTER1	-3.26	-2.50	-0.48	-3.20
COST1	-5.46	-2.55	-0.84	-3.42
YARDST1	-3.35	-2.56	-0.10	-0.64
LEGAL1	-3.10	-3.09	-0.25	-2.15
JOURNAL1	-9.41	-7.77	-0.84	-6.01
MONOGR1	-6.22	-4.04	-0.34	-1.92
EDBOOK1	-9.45	-4.99	-0.62	-2.82
COURT1	-7.26	-5.44	-0.54	-3.48
WORKP1	-6.57	-3.30	-0.57	-2.50
OTHER2	-0.31	-0.13	0.22	0.79
PBEFOR2	-0.21	-0.22	0.17	1.49
PWAR2	-0.44	-0.19	0.25	0.95
PAFTER2	0.20	0.18	0.16	1.27
COST2	-0.35	-0.23	0.20	1.14
YARDST2	0.23	0.19	0.20	1.45
LEGAL2	0.57	0.54	0.11	0.89
JOURNAL2	-0.56	-0.44	0.22	1.52
MONOGR2	-0.68	-0.47	0.18	1.06
EDBOOK2	0.64	0.41	0.45	2.52
COURT2	-1.63	-1.18	0.09	0.57
WORKP2	-1.53	-0.82	0.02	0.12
SPEECH2	-4.64	-0.88	-0.26	-0.42

B2. Controlling for sample selection bias (Continued)

	Linear regression		Log-linear regression	
	Coefficients	Student-t	Coefficients	Student-t
OTHER3	4.48	1.85	0.59	2.10
PBEFOR3	1.14	1.17	0.23	2.05
PWAR3	1.40	0.48	0.39	1.18
PAFTER3	0.73	0.56	0.25	1.65
COST3	2.57	1.38	0.41	1.90
YARDST3	1.09	0.76	0.26	1.57
LEGAL3	0.11	0.09	0.10	0.72
JOURNAL3	9.97	7.74	0.73	4.90
MONOGR3	9.32	6.43	0.63	3.78
EDBOOK3	9.11	5.36	0.79	4.06
COURT3	9.22	6.02	0.59	3.33
WORKP3	8.98	4.63	0.50	2.21
SPEECH3	6.03	1.53	0.32	0.71
OTHER4	12.49	5.53	0.92	3.53
HISTOR4	9.48	2.03	1.04	1.93
PBEFOR4	10.05	7.89	0.64	4.38
PWAR4	6.65	2.38	0.61	1.90
PAFTER4	9.73	6.50	0.65	3.76
COST4	11.23	3.91	0.70	2.10
YARDST4	9.89	6.31	0.68	3.76
LEGAL4	8.63	5.70	0.50	2.87
JOURNAL4	15.64	11.33	0.75	4.74
MONOGR4	13.44	8.09	0.62	3.22
EDBOOK4	21.18	12.95	1.14	6.04
COURT4	13.20	8.14	0.55	2.96
WORKP4	14.86	6.90	0.50	2.00
SPEECH4	10.73	2.70	0.41	0.89

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